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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

SCIENCE, COMMON SENSE AND DECENCY[1]

By Dr. IRVING LANGMUIR

ASSOCIATE DIRECTOR OF THE GENERAL ELECTRIC RESEARCH LABORATORIES

UP to the beginning of the present century one of the main goals of science was to discover natural laws. This was usually accomplished by making experiments under carefully controlled conditions and observing the results. Most experiments when repeated under identical conditions gave the same results.

The scientist, through his own experiments or from previous knowledge based on the work of others, usually developed some theory or explanation of the results of his experiments. In the beginning this might be a mere guess or hypothesis which he would proceed to test by new types of experiments. If a satisfactory theory is obtained which seems in accord with all the data and with other known facts, the solution or goal of the investigation was considered to have been reached.

A satisfactory theory should make possible the predictions of new relationships or the forecasting of the results of new experiments under different conditions. The usefulness of the theory lies just in its ability to predict the results of future experiments. The extraordinary accomplishments of the great mathematical physicists in applying Newton's Laws to the

[1] Address of the retiring president of the American Association for the Advancement of Science. Owing to the postponement of the New York meeting, at the request of the Office of Defense Transportation, the address was broadcast by Science Service over the Columbia Broadcasting System on December 26, 1942.

motions of the heavenly bodies gave scientists of more than a century ago the conviction that all natural phenomena were determined by accurate relations between cause and effect. If the positions, the velocities and the masses of the heavenly bodies were given it was possible to predict with nearly unlimited accuracy the position of the bodies at any future time. The idea of causation, or a necessary relation of cause and effect, has long been embedded in the minds of men. The recognized responsibility of the criminal for his acts, the belief of the value of education and thousands of words in our language all show how implicitly we believe in cause and effect. The teachings of classical science, that is, the science up to 1900, all seem to reinforce this idea of causation for all phenomena.

Philosophers, considering many fields other than science, were divided in their opinions. Many went so far as to believe that everything was absolutely fixed by the initial conditions of the universe and that free will or choice was impossible. Others thought that cause and effect relations were mere illusions.

From the viewpoint of the early classical scientist the proper field for science was unlimited. Given sufficient knowledge, all natural phenomena, even human affairs, could be predicted with certainty. Ampere, for example, stated that if he were given the positions and velocities of all the atoms in the universe it should be possible theoretically to determine the whole future history of the universe. Practically, of course, such predictions would be impossible because we could never hope to get the necessary knowledge nor the time to carry out such elaborate calculations.

A little later scientists developed the kinetic theory of gases according to which the molecules of a gas are moving with high velocity and are continually colliding with one another. They found that the behavior of gases could be understood only by considering the average motions of the individual molecules. The particular motion of a single molecule was of practically no importance. They were thus taught the value of statistical methods, like those which the insurance company now uses to calculate the probable number of its policy-holders that will die within a year.

The theories or explanations which were developed in connection with the natural laws usually involved a description in terms of some kind of a model. In general, instead of thinking of the whole complex world we select only a few elements which we think to be important and concentrate our minds on these. Thus, the chemist developed the atomic theory according to which matter was made up of atoms of as many different kinds as there are chemical elements. These were thought of as small spheres, but no thought was given as to the material of which they were made. When later theories indicated that these atoms were built up of electrons and positive nuclei this made very little difference to the chemist, for he had not needed previously to consider that aspect of the model.

High-school boys to-day are asked to build model airplanes. These must be of such shape that the different types of airplanes can be recognized when the profiles of the models are seen against a white background. It naturally is not particularly important just what kind of material is used in constructing them. Airplane designers build model airplanes to be studied in wind tunnels, but these do not need to be provided with motors.

Most of the models which the scientist uses are purely mental models. Thus, when Maxwell developed the electromagnetic theory by which he explained the properties of light he thought of a medium through which these waves traveled. This was called the ether. It was supposed to have properties like those of elastic solid bodies. The reason for this choice of a model was that at that time the average scientist had been taught in great detail the theory of elasticity of solid bodies. Thus the magnetic and electric fields could be understood in terms of the familiar elastic properties. At the present time relatively few students are well trained in the theories of elasticity. The situation is thus reversed and to-day we explain the properties of elastic solids in terms of the electrical forces acting between their atoms.

Every student of geometry constructs a mental model when he thinks of a triangle. The mathematical lines that bound the triangle are supposed to have no thickness. In other words, they are stripped of any properties except those that we wish particularly to consider.

Most of the laws of physics are stated in mathematical terms, but a mathematical equation itself is a kind of model. We establish or assume some correspondence between things that we measure and the symbols that are used in an equation, and then, after solving the equation so as to obtain a new relation, we see if we can establish a similar correspondence between the new relation and the experimental data obtained from the experiment. If we succeed in this we have demonstrated the power of the mathematical equation to predict events.

The essential characteristic of a model is that it shall resemble in certain desired features the situation that we are considering. On this basis we should recognize that practically any theory has many arbitrary features and has limitations and restrictions imposed by the simplifications that we have made in the development of the theory or the construction of our model.

Beginning with Einstein's relativity theory and Planck's quantum theory a revolution in physical

thought has swept through science. Perhaps the most important aspect of this is that the scientist has ceased to believe that words or concepts can have any absolute meaning. He is not often concerned with questions of existence; he does not know what is the meaning of the question, "Does an atom really exist?" The definition of "atom" is only partly given in the dictionary. Its real meaning lies in the sum total of knowledge on this subject among scientists who have specialized in this field. No one has been authorized to make an exact definition. Furthermore, we can not be sure just what we mean even by the word "exist." Such questions are largely metaphysical and in general do not interest the modern scientist. Bridgman has pointed out that all concepts in science have value only in so far as they can be described in terms of operations or specifications. Thus it doesn't mean much to talk about length or time unless we agree upon the methods by which we are to measure length and time.

For many years, up to about 1930, the new physics based on the quantum theory seemed to be fundamentally irreconcilable with the classical physics of the previous century. Through the more recent development of the uncertainty principle, developed by Bohr and Heisenberg, this conflict has now disappeared. According to this principle it is fundamentally impossible to measure accurately both the velocity and the position of any single elementary particle. It would be possible to measure one or the other accurately but not both simultaneously. Thus it becomes impossible to predict with certainty the movement of a single particle. Therefore, Ampere's estimate of the scope of science has lost its basis.

According to the uncertainty principle, which is now thoroughly well established, the most that can be said about the future motion of any single atom or electron is that it has a definite probability of acting in any given way. Probability thus becomes a fundamental factor in every elementary process. By changing the conditions of the environment of a given atom, as, for example, by changing the force acting on it, we can change these probabilities. In many cases the probability can be made so great that a given result will be almost certain. But in many important cases the uncertainty becomes the dominating feature just as it is in the tossing of a coin.

The net result of the modern principles of physics has been to wipe out almost completely the dogma of causation.

How is it, then, that classical physics has led to such definite clean-cut laws? The simplest answer is that the classical physicist naturally chose as the subjects for his studies those fields which promised greatest success. The aim of the scientist in general was to discover natural laws. He therefore carried on his experiments in such a way as to find the natural laws, for that is what he was looking for. He was best able to accomplish this by working with phenomena which depended upon the behavior of enormous numbers of atoms rather than upon individual atoms. In this way the effects produced by individual atoms averaged out and became imperceptible. We have many familiar examples of this effect of averaging—the deaths of individual human beings can not usually be predicted, but the average death rate in any age group is found to come close to expectation.

Since the discovery of the electron and the quantum and methods of detecting or even counting individual atoms, it has been possible for scientists to undertake investigations of the behavior of single atoms. Here they have found unmistakable experimental evidence that these phenomena depend upon the laws of probability and that they are just as unpredictable in detail as the next throw of a coin. If, however, we were dealing with large numbers of such atoms the behavior of the whole group would be definitely determined by the probability of the individual occurrence and therefore would appear to be governed by laws of cause and effect.

Just as there are two types of physics, classical physics and quantum physics, which have for nearly twenty-five years seemed irreconcilable, just so must we recognize two types of natural phenomena. First, those in which the behavior of the system can be determined from the average behavior of its component parts and second, those in which a single discontinuous event (which may depend upon a single quantum change) becomes magnified in its effect so that the behavior of the whole aggregate does depend upon something that started from a small beginning. The first class of phenomena I want to call *convergent phenomena,* because all the fluctuating details of the individual atoms average out giving a result that converges to a definite state. The second class we may call *divergent phenomena,* where from a small beginning increasingly large effects are produced. In general then we may say that classical physics applies satisfactorily to convergent phenomena and that they conform well to the older ideas of cause and effect. The divergent phenomena on the other hand can best be understood on the basis of quantum theory of modern physics.

Let me give some illustrations of divergent phenomena. The wonderful cloud chamber experiments of C. T. R. Wilson show that a single high-speed electron or an alpha particle from an atom of radium, in passing through a gas, leaves a trail of ions. By having moisture in the gas and by causing the gas to expand just after these ions are produced, drops of water are made to condense on the ions. By a strong illumination it thus becomes possible to see or photo-

graph this track of ions as a white line on a dark background. The time at which an alpha particle will be emitted from a radium atom is inherently unpredictable. It would be totally contrary to the teachings of modern physics to suppose that our inability to predict when such an event is merely due to our ignorance of the conditions surrounding the particular atom. The uncertainty principle requires that even if these conditions were absolutely fixed the time of emission and the direction of emission of the alpha particle are subject to the laws of chance and thus for a single particle are unpredictable.

The occurrences in the Wilson cloud chamber following the disintegration of a single radium atom are typical divergent phenomena. The single quantum event led to the production of countless thousands of water droplets and these made the track of the alpha particle visible and led to its reproduction in a photograph. This track may show some unusual feature of particular interest to the scientist; for example, it may have a kink which indicates that the alpha particle collided with the nucleus in one of the molecules of gas. The photograph may therefore be published —may start discussions among scientists that involve thousands of man hours—may delay one of them so that he misses a train on which he might otherwise have suffered a fatal accident. Examples of this kind, any number of which could be given, show that it is possible for single unpredictable quantum events to alter the course of human history.

The formation of crystals on cooling a liquid involves the formation of nuclei or crystallization centers that must originate from discrete, atomic phenomena. The spontaneous formation of these nuclei often depends upon chance.

At a camp at Lake George, in winter, I have often found that a pail of water is unfrozen in the morning after being in a room far below freezing, but it suddenly turns to slush upon being lifted from the floor.

Glycerine is commonly known as a viscous liquid, even at low temperatures. Yet if crystals are once formed they melt only at 64° F. If a minute crystal of this kind is introduced into pure glycerine at temperatures below 64° the entire liquid gradually solidifies.

During a whole winter in Schenectady I left several small bottles of glycerine outdoors and I kept the lower ends of test-tubes containing glycerine in liquid air for days, but in no case did crystals form.

My brother, A. C. Langmuir, visited a glycerine refinery in Canada which had operated for many years without ever having any experience with crystalline glycerine. But suddenly one winter, without exceptionally low temperatures, the pipes carrying the glycerine from one piece of apparatus to another froze up. The whole plant and even the dust on the ground became contaminated with nuclei and although any part of the plant could be temporarily freed from crystals by heating above 64° it was found that whenever the temperature anywhere fell below 64° crystals would begin forming. The whole plant had to be shut down for months until outdoor temperatures rose above 64°.

Here we have an example of an inherently unpredictable divergent phenomenon that profoundly affected human lives.

Every thunderstorm or tornado must start from a small beginning and at least the details of the irregular courses of such storms across the country would be modified by single quantum phenomena that acted during the initial stages. Yet small details such as the place where lightning strikes or damage occurs from a tornado may be important to a human being.

Much more obvious examples of divergent phenomena which affect human life are those involved in the mechanism of heredity and the origin of species. Whether the genes are inherited from the mother or from the father seems to be fundamentally a matter of chance, undoubtedly involving changes in single atoms. It is known definitely that changes in genes or mutations can be produced by x-rays, and it has even been proved that a single quantum is sufficient to bring about such an alteration. The growth of any animal from a single cell is a typical illustration of a divergent phenomenon. The origins of species and all evolutionary processes, involving as they do natural selection acting upon mutations, must depend at almost every stage upon phenomena which originate in single atoms.

An idea that develops in a human brain seems to have all the characteristics of divergent phenomena. All through our lives we are confronted with situations where we must make a choice and this choice may sometimes alter the whole future course of our lives. Occasionally such decisions are made by tossing a coin —an event which seems unpredictable—but there is no reason to doubt that single atomic processes, or small groups of them which follow statistical laws, may not often be a determining element.

When certain bacteria are heated until they begin to die it is found that in successive intervals of time the same fraction of the survivors succumb. This seems to prove that the life of these cells depends on a single unstable molecule whose change involves death. It is thus a matter of pure chance as to which particular individuals die within a given period.

The examples that I have given of convergent and of divergent phenomena are purposely chosen as extreme types. Actually there are many intermediate cases that are not clearly one or the other. Or, again, phenomena which start out to be divergent may ap-

parently degenerate into the convergent type. For example, if the photograph of the Wilson cloud track did not prove to be of interest it would soon be forgotten and might have no further ascertainable effects on human lives. Similarly in the evolutionary process a new species that originates as a single divergent phenomenon may not survive and its effect soon seems to die out.

We must recognize, nevertheless, that a divergent phenomenon which once occurs permanently alters details of molecular arrangements in the convergent systems that follow it and thus conditions may be brought about which favor the occurrence of new divergent phenomena. In a world in which divergent or quantum phenomena occur we can thus have no absolute relation of cause and effect.

As the implications of the uncertainty principle, especially as applied to divergent phenomena, are more generally recognized the limitation of the idea of causality should have profound effects on our habits of thought. The science of logic itself is involved in these changes. Two of the fundamental postulates of logic are known as the law of uniformity of nature and the law of the excluded middle. The first of these laws is equivalent of the postulate of causality in nature. The second law is simply the familiar postulate that a given proposition must be either true or false. In the past these so-called laws have formed the basis of much of our reasoning. It seems to me, however, that they play no important part in the progress of modern science. The cause and effect postulate is only applicable to convergent phenomena. The second postulate in assuming that any proposition must be true or false implies that we attach absolute meanings to words or concepts. If concepts have meanings only in terms of the operations used to define them we can see that they are necessarily fuzzy. Take, for example, this statement, "Atoms are indestructible." Is this true or false? The answer depends upon what aspect of atoms is considered. To the chemist the statement is as true as it ever was. But a physicist, studying radioactive changes, recognized that some atoms undergo spontaneous disintegration or destruction. The fact is that the chemist and the physicist have no exact definitions of the word "atom" and they also do not know in any absolute sense what they mean by "indestructible."

Fortunately such questions no longer occupy much of the time of scientists, who are usually concerned with more concrete problems which they endeavor to treat in common-sense ways.

It is often thought by the layman, and many of those who are working in so-called social sciences, that the field of science should be unlimited, that reason should take the place of intuition, that realism should replace emotions and that morality is of value only so far as it can be justified by analytical reasoning.

Human affairs are characterized by a complexity of a far higher order than that encountered ordinarily in the field of science.

To avoid alternating periods of depression and prosperity economists propose to change our laws. They reason that such a change would eliminate the cause of the depressions. They endeavor to develop a science of economics by which sound solutions to such problems can be reached.

I believe the field of application of science in such problems is extremely limited. A scientist has to define his problem and usually has to bring about simplified conditions for his experiments which exclude undesired factors. So the economist has to invent an "economic man" who always does the thing expected of him. No two economists would agree exactly upon the characteristics of this hypothetical man and any conclusions drawn as to his behavior are of doubtful application to actual cases involving human beings. There is no logical scientific method for determining just how one can formulate such a problem or what factors one must exclude. It really comes down to a matter of common sense or good judgment. All too often wishful thinking determines the formulation of the problem. Thus, even if scientifically logical processes are applied to the problem, the results may have no greater validity than that of the good or bad judgment involved in the original assumptions.

Some of the difficulties involved in a scientific approach to economic problems is illustrated by the following: If we wish to analyze the cause of a depression (or for example, a war) we should ask ourselves what we mean by the word "cause" in this connection. In terms of operations the usual meaning of the word cause is something as follows: It is a common experience, in a study of convergent phenomena, that if a given set of physical conditions are brought about repeatedly at different times, the same result occurs in each case. Except in so far as it is possible to repeat the experiment and get the same result it is impossible to give a definite meaning to the word cause.

In the case of a depression or a war, we logically need to produce, or at least to observe, a given set of possible antecedent conditions and to see whether they are always followed by depressions. Since we can not produce experimental depressions, nor have we sufficient observational data to enable us by statistical means to unravel the enormous number of factors involved, we must conclude that the word "cause" as applied to a depression has an extremely fuzzy meaning.

When we consider the nature of human affairs it is to me obvious that divergent phenomena frequently

play a role of vital importance. It is true that some of our historians cynically taught most of our college students from 1925 to 1938 that wars, the rise and fall of a nation, etc., were determined by nearly cosmic causes. They tried to show that economic pressure, and power politics on the part of England or France, etc., would have brought the same result whether or not Kaiser Wilhelm or Hitler or any other individual or group of individuals had or had not acted the way they did. Germany, facing the world in a realistic way, was proved, almost scientifically, to be justified in using ruthless methods—because of the energy and other characteristics of the German people they would necessarily acquire and should acquire a place in the sun greater than that of England, which was already inevitably on the downward path.

I can see no justification whatever for such teaching that science proves that general causes (convergent phenomena) dominate in human affairs over the results of individual action (divergent phenomena). It is true that it is not possible to prove one way or the other that human affairs are determined primarily by convergent phenomena. The very existence of divergent phenomena almost precludes the possibility of such proof.

The mistaken over-emphasis on convergent phenomena in human affairs and the reliance on so-called scientific methods have been responsible in large degree for much of the cynicism of the last few decades.

The philosophy which seems to have made the German people such willing aggressors is allegedly based upon scientific realism. Almost any system of morality or immorality could receive support from the writings of Nietzsche, so inconsistent are they with one another. But his teachings, which purport to be based on the laws of natural selection, have led in Germany to a glorification of brute strength, with the elimination of sympathy, love, toleration and all existing altruistic emotions.

Darwin, himself, however, recognized that the higher social, moral and spiritual developments of mankind were factors which aided in survival. This is often referred to loosely as the law of the survival of the fittest. The concept of fitness seems, however, inherently rather fuzzy. Apparently those individuals are fittest which possess characteristics that increase the probability that they shall survive.

We often hear realists deplore the effects of charity which tend to keep the unfit alive. We are even told that the whole course of evolution may be revised in this way. Similar arguments could be used against the surgeon who removes an appendix or a doctor who uses a sulfa drug to cure pneumonia.

But what is the need of developing a race immune to appendicitis or pneumonia if we possess means for preventing their ill effects. The characteristics that determine fitness merely change from those of immunity to those which determine whether a race is able to provide good medical treatment.

The coming victory of the United Nations will prove that survival of a nation may be prevented by an aggressive spirit, by a desire to conquer or enslave the world, or by intolerance, ruthlessness and cruelty. In fact, there is no scientific reason why decency and morality may not prove to be vastly more important factors in survival than brute strength.

In spite of the fact that we can no longer justify a belief in absolute causation and must recognize the great importance of divergent phenomena in human life we still have to deal with causes and effects. After all we must plan for the future. We can do this, however, by estimating probabilities even where we do not believe that definite results will inevitably follow. When our army lands in North Africa its probable success depends on the carefulness of the preparations and the quality of the strategy. But no amount of foresight can render success absolutely certain.

It does not seem to me that we need be discouraged if science is not capable of solving all problems even in the distant future. I see no objections to recognizing that the field of science is limited.

In the complicated situations of life we have to solve numerous problems and make many decisions. It is absurd to think that reason should be our guide in all cases. Reason is too slow and too difficult. We do not have the necessary data or we can not simplify our problem sufficiently to apply the methods of reasoning. What then must we do? Why not do what the human race always has done—use the abilities we have—use common sense, judgment and experience. We under-rate the importance of intuition.

In almost every scientific problem which I have succeeded in solving, even after those that have taken days or months of work, the final solution has come to my mind in a fraction of a second by a process which is not consciously one of reasoning. Such intuitive ideas are often wrong. The good must be weeded out from the bad—sometimes by common sense or judgment—other times by reasoning. The power of the human mind is far more remarkable than one ordinarily thinks. We can often size up a situation, or judge the character of a man by the expression of his face or by his acts in a way that would be quite impossible to describe in words.

People differ greatly in their ability to reach correct conclusions by such methods. Our numerous superstitions and the present popularity of astrology prove how often our minds make blunders. Since we have to live with our minds, we should train them, develop them, censor them—but let us not restrict them by trying to regulate our lives solely by science or by reason.

Our morality is a kind of summation of the wisdom and experience of our race. It comes to us largely by tradition or religion. Some people justify evil things on the basis of morality—but by and large a recognition of right and wrong, even if these concepts are sometimes fuzzy, has proved to be of value to mankind. The philosophical, metaphysical or even scientific analysis of the principles of ethics has not proved particularly fruitful. A sense of morality and decency, even if not scientific, may help win the war.

THE AMERICAN ETHNOLOGICAL SOCIETY[1]

By the late Dr. FRANZ BOAS

PROFESSOR EMERITUS OF ANTHROPOLOGY, COLUMBIA UNIVERSITY

THE American Ethnological Society was founded at a period when interest in racial and ethnic questions was very lively. The racial question was particularly a subject of heated discussion on account of the struggle between the abolitionists and the defendants of slavery. It had taken the form of a passionate controversy between those who stood for the unity of mankind and those who claimed distinct origins for the races of man. At the same time the interest in the customs and lives of alien people was very lively. It was about the time of the Wilkes expedition to the South Seas, of the vast collection of material on the American Indians by Schoolcraft and of intensive interest in the archeology of our continent.

Through the energy and interest of Albert Gallatin the American Ethnological Society was established and during the years from 1842 until his death in 1849 the society was a center of anthropological interest in New York City. About the same time the Government explorations of the western part of the United States gave strong stimulus to interest in the study of Indian tribes and of American antiquities. For a number of years the society contributed in important ways to our knowledge of both North and Central America. The investigations of members of the society were published in the form of Transactions, of which three volumes appeared, in 1845, 1848 and 1853. The last one of these volumes was never published because the whole edition was destroyed by fire before it was distributed. It was, however, republished in photographic reproduction in 1909 by the revived society. A new start was made in 1860 when the society began publication of a bulletin containing brief communications, the last of which appeared in 1869. The interest of members in the society was evidently declining rapidly and in 1869 a committee headed by E. George Squier issued a statement calling for reorganization on a new basis. The outlook of this group is characterized by the following statement issued in 1869, evidently drafted by Mr. Squier:

[1] Address delivered at the celebration of the centenary of the society, on November 14, 1942. Dr. Boas died on December 21.

Statesmen, whether senators or kings, can no longer overlook the profound lessons inculcated by anthropology. The political reorganization of Europe is going on in consonance with its discoveries and results. Religion under its influence is separating itself from a ritualistic dogmatism that has nothing to do with morals or the relationship that exists between men and God and has become all the loftier from the dissociation.

To these grand results we may ask what has the American Ethnological Society contributed. Absolutely, for twenty years, nothing. True, ten of these years have been unfavorable to scientific pursuits in this country. Students having common sympathies and aims have been separated by political and social barriers and investigators weaned or diverted from their pursuits by imperative requirements in other fields. Estranged co-laborers, however, are returning with that catholic spirit which study for Truth inspires and encourages, to their old associations and researches; and the altered condition of our common country encourages and, indeed, makes necessary a wider and deeper investigation of the character and true relations of the varieties and races of mankind than ever existed before. But this investigation must be made *ab initio*, or rather in a purely abstract scientific sense. It can not be done by men who, for any reason or motive, bring into the study the element of faith, or adhesion to dogmas or creeds of any kind whatever. These subtle elements of depression of scientific inquiry have been, to a certain degree, the ruin of this Society. Your reporter can remember when the question of human unity could not be discussed without offense to some of the members of the Society and when its casual introduction was made a ground of impassioned protest. This allusion is made only to enforce the vital truth that, in scientific inquiry, the item of faith must be entirely eliminated. Not having been so, discussion in this Society has been relatively tame and fruitless.

On the basis of this critical examination of the activities of the society during the two decades from 1850 to 1869, the American Ethnological Society was dissolved and it was decided to reconstitute the society as the American Anthropological Society. Since at the same time an association with the same name had been founded in Boston, the committee followed the example of the newly consolidated Anthropological and Ethnological Societies of London and adopted the name "The Anthropological Institute of New

York," which published its first volume in 1871–1872. The object of the newly organized group was stated as "the study of man in all his varieties and under all his aspects and relations."

This attempt at reorganization was evidently unsuccessful. An association of the old members of the Ethnological Society with the newly founded American Museum of Natural History led to a formal continuance of activities under the old name of the American Ethnological Society, but there was evidently no productive scientific activity during this period.

About 1895, when the American Museum of Natural History began its field explorations and when anthropology was introduced as a subject of instruction in Columbia University, a number of younger men were drawn to New York who met occasionally in an informal way to discuss anthropological problems. The first attempts to affiliate this group with the surviving members of the American Ethnological Society were unsuccessful because the society had practically become a group which met occasionally for social purposes. As the American Museum of Natural History developed, the proposal of reactivating the American Ethnological Society was renewed. In 1900 the younger group joined the society, and the president of the American Museum of Natural History, Morris K. Jesup, was elected president of the society. He was succeeded by General James Grant Wilson, who served until 1913. After this time the officers of the society were confined entirely to professional anthropologists.

About this time cooperation of the scientific societies of New York was established under the leadership of the New York Academy of Sciences. The Ethnological Society joined this organization and since that time its regular meetings have been held in connection with the meetings of the New York Academy of Sciences at the American Museum of Natural History.

With the renewal of the activities of the society it was felt that the most important service that could be rendered was to establish a series of publications and the society became essentially a publishing society, although regular meetings were held, without however any attempt to publish these in the form of transactions. The meetings were on the whole rather informal and devoted to questions on which the members of the society or visiting anthropologists were working.

It was a period of a general reorganization of anthropological societies. Long before this time anthropology had been recognized as one of the sections of the American Association for the Advancement of Science, and in the annual meetings anthropologists from all parts of the country used to meet and to discuss their problems. About the same time an anthropological society was founded in Washington, curiously enough followed by the establishment of a women's anthropological society in the same city. On account of the difficulties of publication it was felt that a general anthropological society was needed and at the time of the meeting of the American Association for the Advancement of Science in Pittsburgh in 1902, the establishment of the American Anthropological Association was decided upon. The difficult financial question of establishing an adequate journal for the publication of work of American anthropologists was met by the generosity of Charles P. Bowditch of Boston and the Duke of Loubat of New York, who helped the journal during the early years of its existence. At the same time an agreement was reached between the American Anthropological Association and the American Ethnological Society for a certain division of the kind of publication to be maintained by either society. The *American Anthropologist* was established as the joint journal of both societies. While the *American Anthropologist* was to contain papers on general anthropology the Ethnological Society confined its publication to a series of volumes giving ethnological records of various tribes in the original language with translations, thus following the earlier series published by the U. S. Geological and Geographical Survey of the Rocky Mountain Region and of the series of native documents published by Daniel Garrison Brinton.

During these years of specialization the *Journal of American Folk-Lore* had been established owing to the energetic efforts of William Wells Newell. In 1918, a special journal was established by Dr. Aleš Hrdlička for physical anthropology and the *International Journal of American Linguistics* by myself in 1917. In 1940 the American Ethnological Society started a new series of brief ethnographic monographs.

Thus the society has become an active member in anthropological work in our country. Let us hope that it will continue its active participation in anthropological work and contribute by the researches of its members and by its more popular activities to the solution of the difficult social problems of our times.

OBITUARY

JOHN FRANKLIN DANIEL

PROFESSOR JOHN FRANKLIN DANIEL, chairman of the department of zoology of the University of California, died in Berkeley on November 2, 1942. The students, colleagues, friends and family of Dr. Daniel assembled at his home on the afternoon of November

third for a brief service in memory of him and his long, fruitful life.

He was born on July 31, 1873, in O'Fallon, Missouri. His education beyond the secondary school was begun at Southern Illinois State Normal University. As he completed his second year of college the U. S. Government issued a call for teachers to go to the Philippine Islands. Daniel volunteered and was a member of the first group to be sent there. The four years in the Philippines (1901–1905) were among the most important of his life. It was probably here that he learned how to teach. His first pupils were the unsympathetic, unschooled natives, most of whom spoke no English; the facilities for instruction were meager; but by trial and error, instructor and pupil learned from each other. Before Daniel left the Archipelago he had become district superintendent of schools for the island of Cebu. The contribution of these early American teachers becomes more real and more significant as we observe the loyalty of the Philippine people to American ideals in the recent and tragic history.

The interruption in his formal education did not lessen Daniel's interest in zoology. The simple book on the animal life of Malaysia which he wrote while in the Philippines is good evidence. Serious study, however, was resumed at the University of Chicago, from which he received the bachelor's degree in 1906, and at Johns Hopkins University, where the doctorate was completed in 1908. The same year he married Menetta Brooks, daughter of William Keith Brooks. The following year was spent at the Pasteur Institute at Lille, France, where Daniel held the Adam T. Bruce fellowship. On returning to the United States he accepted an instructorship in zoology at the University of Michigan. In 1910 he joined the faculty in zoology at the University of California at Berkeley. Here he served for thirty-two years, the last six as chairman of the department. The honors which he prized most were his appointment as United States delegate to the International Congress of Zoology at Lisbon in 1935 and his decoration in 1936 by France as Chevalier of the Légion d'honneur.

Dr. Daniel contributed about equally in teaching, research and administration. Through his course in general chordate zoology, which he gave for many years, his influence was felt by hundreds of undergraduate students. The graduate students of the department and especially his own students are deeply indebted to him. From him they learned to appreciate the sacredness of character, the value of high scholarship, the importance of being a gentleman, the dignity of the teaching profession and the opportunities and responsibilities of a great university. From him they learned that industry is a privilege; that generosity and kindness are everlasting virtues; that truth is both an anchor and a compass.

Dr. Daniel's contributions in research were made in two fields: experimental morphogenesis and comparative anatomy. The subject of his doctoral work, carried out under the direction of Professor H. S. Jennings, was the "Adaptation and Immunity of Lower Organisms to Ethyl Alcohol." It was probably through this and an earlier study on the adjustment of *Paramecium* to distilled water that Dr. Daniel became devoted to the experimental method. A short paper appeared soon after his return from the Pasteur Institute at Lille on the length of the period of gestation in lactating and in primiparous mice. He demonstrated that females bearing young while suckling an older litter have a considerably longer period of gestation than those carrying their first litter. Had this line of inquiry been continued, Dr. Daniel might well have become an endocrinologist. However, in accepting an instructorship at the University of California his attention was turned to comparative anatomy in general and the elasmobranch fishes in particular. A series of papers on the morphology of these fishes culminated in his widely known monograph, "The Elasmobranch Fishes," published in 1922. His feeling for these creatures is beautifully expressed in the introduction to his book:

> There lives to-day a vast group of fishes, some of which are littoral, keeping close to shore; others are the nomads of the ocean, roaming vast expanses of its waters; others there are which are pelagic, living near its surface; and still others that are the inhabitants of the profound depths into which the sunlight never penetrates—these are the sharks, to the man with nets the most worthless, to the naturalist among the most interesting of living things.

The work on the elasmobranch fishes concluded his studies in the field of comparative anatomy except for two papers published later on the larval cyclostome, Ammocoetes; one of these, a report of the circulatory system, is a valuable supplement to his monograph on the elasmobranchs. Dr. Daniel's interest in experimental biology was revived by the discoveries of Spemann and a visit to Spemann's laboratory in 1929. The final segment of his scientific career was devoted to a study of the problems of pattern in the amphibian egg. He was especially interested in the distribution of pigment and yolk platelets in the egg, the relationship of the zygotic pattern to that of the future embryo and the nature and importance of the secondary investments of the egg—the chorion and the layers of jelly.

Dr. Monroe E. Deutsch, provost of the University of California and a close friend of Dr. Daniel's, recently stated that an epitome of Daniel as an administrator would be the one word—*just*. Dr.

Daniel's experience in university administration began long before he became head of the department of zoology. He rendered noteworthy service as chairman of some of the important faculty committees such as the committee on budget and inter-departmental relations and the library committee. In the opinion of the author, Dr. Daniel's most significant contribution as chairman of his department was his interest in and encouragement of young men and women of high scholarship. His graduate assistants were selected with exceeding care and their apprenticeship in teaching and development as scientists and scholars were carefully studied. To them he was personally devoted, far more, perhaps, than they will ever know. In his passing they have lost a friend and a counselor; he survives, however, a symbol and a creed.

RICHARD M. EAKIN
UNIVERSITY OF CALIFORNIA, BERKELEY

RECENT DEATHS

DR. FRANZ BOAS, professor emeritus of anthropology of Columbia University, died on December 21 at the age of eighty-three years.

DR. FRANK DAWSON ADAMS, Logan professor of geology at McGill University from 1894 to 1931 and emeritus vice-principal of the university, died on December 26 at the age of eighty-four years.

DR. JABEZ HENRY ELLIOTT, president of the American Association of the History of Medicine and professor of the history of medicine at the University of Toronto, died on December 18. He was sixty-nine years old.

DR. WILLIAM MARTIN BLANCHARD, professor of chemistry and dean emeritus of the DePauw University College of Liberal Arts, died on December 21. He was sixty-eight years old.

DR. HANS G. BEUTLER, research associate in physics at the University of Chicago, died on December 15 at the age of forty-six years. Dr. Beutler, who came to this country in 1936 from the Kaiser Wilhelm Institute for Physical Chemistry in Berlin, was a spectroscopist.

SCIENTIFIC EVENTS

TERCENTENARY OF THE BIRTH OF ISAAC NEWTON[1]

WHEN the tercentenary of the birth of Sir Isaac Newton was celebrated by Fellows of the Royal Society, in the Royal Institution, in December, Sir Henry Dale, president of the society, announced the successful conclusion of negotiations to acquire and preserve the birthplace "of the greatest of our men of science." The Pilgrim Trust, he said, will be responsible for the sum required for the purchase, which the Lord of the Manor of Woolsthorpe (Lincolnshire) has agreed to at a price substantially less than its value.

Sir Henry Dale described how

in the hamlet of Woolsthorpe, near Colsterworth, on the Great North Road, some six miles south of Grantham, there is still a modest manor farmhouse, with a small orchard in front of it. Here the Newtons lived, simple yeoman farmers, and here, two months after his father had died, Isaac Newton was born, a puny, premature infant, on Christmas Day, 1642, 20 years before the Royal Society was incorporated by the grant of its first charter. The house stands but little altered since that day. The room in which Newton was born has a simple marble tablet on the wall, inscribed with Pope's well-known couplet.

But this house had importance in Newton's later life and in his work, and not only as his birthplace. It was here that he returned from his schooling at Grantham, at the age of 16, to take charge of the farm for his mother; and here, to the incalculable gain of science and the world, he showed such incompetence as a farmer that he was sent back to school and thence to Cambridge. It was here, again, that he returned in the autumn of 1665, when the plague drove him from Cambridge; and here, during the following 18 months of quiet exile in the country, his early ripening genius grasped already the essential principles of his major theoretical discoveries. One can still see the upper chamber which he then used as a study; and in the little orchard there is an old, recumbent apple tree which, one will be told, is descended by direct grafting from that which Newton saw.

The land which Newton's family farmed was rapidly being laid waste by quarrying for iron-stone and soon there would have been little left unspoiled save the orchard and garden round the house. The Royal Society felt that something should be done to preserve for posterity a house and garden which carried such momentous memories, and which had meant so much for science. Accordingly a small committee was formed, in which Sir John Russell and Sir James Jeans joined with the officers of the society to negotiate with the lord of the manor, Major E. B. Turnor, of Ponton Hall, near Grantham, in order to put this tiny but historic property for as long as possible beyond the risk of damage or decay.

[1] The *Times*, London.

ELECTION OF THE PRESIDENT AND OTHER OFFICERS OF THE AMERICAN CHEMICAL SOCIETY

DR. THOMAS MIDGLEY, JR., vice-president of the Ethyl Corporation, known for his discovery of tetraethyl lead which has made possible dramatic advances

in automotive and aircraft engines, has been elected president of the American Chemical Society for 1944.

Dr. Midgley will take office as president-elect on January 1, when Dr. Per K. Frolich, director of the Chemical Division, Esso Laboratories of the Standard Oil Development Company, Elizabeth, N. J., a leader in the development of synthetic rubber, becomes president, succeeding Dr. Harry N. Holmes, head of the department of chemistry at Oberlin College.

Dr. Midgley was chosen by the council from four nominees receiving the largest number of votes in a national mail ballot of approximately 32,000 members of the society. The council includes national officers, directors, editors of the publications, past presidents, the chairmen of eighteen professional divisions and councilors from a hundred local sections, and councilors-at-large.

Dr. Walter A. Schmidt, president of the Western Precipitation Company, Los Angeles, Calif., was elected a director-at-large to succeed Dr. Midgley. Dr. Leason H. Adams, of the Geophysical Laboratory of the Carnegie Institution of Washington, and Professor Robert E. Swain, of Stanford University, were reelected regional directors.

New councilors-at-large are: Dr. M. L. Crossley, director of research of the Calco Chemical Division, American Cyanamid Company, Bound Brook, N. J.; Professor Vincent du Vigneaud, head of the department of chemistry, Cornell University Medical College, New York; Dr. W. Albert Noyes, Jr., professor of physical chemistry in the University of Rochester, and Professor R. L. Shriner, chairman of the department of chemistry of Indiana University.

According to the official announcement of the society:

> Dr. Midgley has won recognition for discoveries which are outstanding both from the standpoint of pioneering in new fields and from the standpoint of commercial importance. His discovery in 1922 of tetraethyl lead as an antiknock agent was made after he and his colleagues in the General Motors Research Laboratories had tried more than 33,000 different chemical compounds without success.
>
> The performance of the modern military and transport plane, it is pointed out, is due in large part to the spectacular development of high-octane gasoline, a development in which tetraethyl lead, now a vital war material, has played an important role.
>
> He has contributed largely to the knowledge of the chemistry of rubber and the methods of synthesizing rubber. With Dr. Albert L. Henne, of the Ohio State University, he developed the organic chlorofluorides which have become widely used as non-inflammable, non-toxic refrigerants. He was associated with the developments connected with the recovery of bromine from sea water.

Dr. Midgley, speaking on the occasion of the award to him of the Willard Gibbs Medal of the Chicago Section of the American Chemical Society on May 22 of this year, stated that "America's acute shortage of rubber must be laid to rubber technologists who have failed to develop practical methods of separating this vital material from plants growing plentifully in our own country."

Dr. Midgley has also been awarded the Priestley Medal of the American Chemical Society, the William H. Nichols Medal of the New York Section, the Perkin Medal of the Society of Chemical Industry and the Longstreth Medal of the Franklin Institute. Wooster College conferred the honorary degree of doctor of science upon him in 1933. He is a member of numerous scientific organizations.

Dr. Midgley was born on May 18, 1889, at Beaver Falls, Pa. His father, Thomas Midgley, an inventor and manufacturing executive, came to the United States from London, England, at the age of six. Dr. Midgley attended the public schools of Ohio and later Betts Academy at Stamford, Conn. He received the degree of mechanical engineer from Cornell University in 1911.

After his graduation he entered the employ of the National Cash Register Company, Dayton, Ohio. Later, with his father, he established the Midgley Tire and Rubber Company of Lancaster, Ohio. In 1916 he returned to Dayton and began work under Dr. Charles F. Kettering, with whom he has since been associated in various activities, including the organization of the General Motors Research Corporation. He is vice-president of Kinetic Chemicals, Inc., chairman of the Board of Directors of the American Chemical Society and vice-president of Ohio State University Research Foundation. He holds about one hundred patents.

AWARD OF THE EDISON MEDAL TO DR. ARMSTRONG

THE Edison Medal for 1942 has been awarded by the American Institute of Electrical Engineers to Dr. Edwin Howard Armstrong, professor of electrical engineering at Columbia University, "for distinguished contributions to the art of electric communication, notably the regenerative circuit, the superheterodyne, and frequency modulation." The medal will be presented to Dr. Armstrong an the evening of January 27, in the Engineering Auditorium, 33 West 39th Street, New York, N. Y., during the national technical meeting of the institute to be held in the Engineering Societies Building from January 25 to 29.

The Edison Medal was founded by associates and friends of Thomas A. Edison, and is awarded annually for "meritorious achievement in electrical science, electrical engineering, or the electrical arts" by a com-

mittee consisting of twenty-four members of the American Institute of Electrical Engineers. A statement issued by the institute reads:

Edwin Howard Armstrong was born in New York City on December 18, 1890. He began his engineering studies at Columbia University and became a protégé of Michael I. Pupin, Edison Medal recipient in 1920, with whom he worked closely on many important research undertakings.

While an undergraduate he became interested in the operating properties of the audion detector and set out to learn more of the principles of operation of thermionic tubes. The research that followed at his home in Yonkers, New York, resulted in his invention of the feedback or regenerative circuit which became the means not only of increasing the sensitivity of the audion as a detector of radio signals but also became the means of producing for the first time continuous high frequency oscillations by means of a thermionic tube. He filed applications on this invention in the latter part of 1913 and a patent was issued to him on October 6, 1914. In December, 1914, he published the first scientifically correct explanation of detection and amplification in the audion detector and in March, 1915, disclosed the regenerative and oscillating circuits.

This keystone of radio development was later to become involved in fourteen years of litigation and which in the end was decided by lay courts based on errors of fact and judgment which were contrary to the scientific facts.

He was graduated from Columbia University in 1913 with a degree of electrical engineer, and continued as an assistant in the department of electrical engineering. In 1915, he received the Trowbridge Fellowship from Columbia University.

In 1917, Armstrong entered the service of the United States Army as a Captain in the Signal Corps, and later was promoted to rank of Major. While serving in this capacity in France, he made his second invention which was destined to be another cornerstone in the development of the radio communication art. This was the superheterodyne receiving system.

This system of receiving far surpassed any development up to that time and it is still the type of circuit used to-day in practically all radio receivers.

The third outstanding invention was his development of the super-regenerative circuit which was disclosed to the art in 1922. This system of radio reception provides a means of increasing the sensitivity of a detector above that normally obtained by means of simple regeneration. This receiving circuit supplied the principal means of exploring and developing the ultra short wave channels.

The fourth outstanding invention of Major Armstrong was the development of wide band frequency modulation in 1933. This system is now recognized as the basis for an entirely new era in radio broadcasting and communication. All the broadcasting companies and many individual broadcasters have either applied to the Federal Communications Commission for licenses or have transmitters in operation using this system. There are 17 stations already on scheduled operation and it is conservatively estimated that 30 or more will be in service by the end of the year. The Federal Communications Commission has over 150 applications pending before it at this time.

This system of radio communication is radically different from the amplitude modulation system now in general use. It provides a means for producing static-free and noise-free signals with a fidelity and tonal range not previously obtained with the present broadcast facilities. In addition to eliminating much of the noise level from broadcast radio programs which are prevalent in urban areas the system provides means for transmitting on several separate channels over one carrier and in addition makes possible the use of the same carrier frequency by a number of stations separated by only a few hundred miles without causing mutual interference on that frequency.

Probably no one man has contributed as many fundamental radio inventions which so closely touch on our everyday life as Major Armstrong. The discovery of the regenerative circuit made possible long distance wireless communication and the building and operation of world-wide communication systems. Then, as broadcasting began to grow, the superheterodyne circuit became the greatest stimulant to the art bringing with it better reception and the attendant increase in enjoyment to millions of listeners. At the same time the manufacture of apparatus and the building and operation of stations brought work and prosperity to thousands. The super-regenerative circuit made practical the first 2-way police communication systems on the ultra short waves and has resulted in more rapid police action in safeguarding property and in the protection of life and limb. Now the frequency modulation system of communication is beginning to give the public a finer radio service and like Major Armstrong's other inventions is destined to add much to the nation's enjoyment and wealth.

Professor Armstrong has received many honors, including the degree of doctor of science from Columbia University in 1929, and from Muhlenberg College in 1941, the Medal of Honor of the Institute of Radio Engineers, 1917, the Egleston Medal of Columbia University, 1939, the Holley Medal of the American Society of Mechanical Engineers, 1940, the Franklin Medal of the Franklin Institute, 1941, and the John Scott Medal, awarded by the Board of Directors of City Trusts, City of Philadelphia, 1942. He was made a Chevalier de la Légion d'honneur by the French Government in 1919. He received one of the nineteen national awards of "Modern Pioneer" by the National Association of Manufacturers in 1940.

SCIENTIFIC NOTES AND NEWS

SIR HENRY DALE was reelected president of the Royal Society at the two hundred and eightieth anniversary meeting which preceded the celebrations of the tercentenary of the birth of Isaac Newton.

DR. EVARTS A. GRAHAM, Bixby professor of surgery, Washington University School of Medicine, St. Louis, was presented with the St. Louis Award during special ceremonies in the mayor's office on November 5. The award is given annually by an anonymous donor to the citizen of St. Louis who has made "the most outstanding contribution to the community during the year." It includes a check for $1,000 and a certificate. The award was made in recognition of Dr. Graham's development of pulmonary and hepatic surgery, his productive leadership as a teacher of students and practitioners and for his influence on surgical theory and practice, and in particular for his "comprehensive focusing of present knowledge on the treatment of war injuries, thereby making this information easily available to all surgeons for the conservation and rehabilitation of the victims of the carnage of war."

EDWIN C. WELLS, principal contributor to the design and engineering work on the flying fortresses manufactured by the Boeing Aircraft Company of Seattle, will receive the Lawrence Sperry Award of the Institute of the Aeronautical Sciences. The citation reads: "For outstanding contributions to the art of airplane design with special reference to four-engined aircraft."

E. MEAD JOHNSON AWARDS for research in pediatrics for 1941 were presented through the American Academy of Pediatrics at its annual meeting in Chicago in November. The prize of $500 was presented to Drs. Howard A. Howe and David Bodian for their work in poliomyelitis, and the prize of $300 went to Drs. Harold E. and Helen C. Harrison for their "painstaking work on the excretion and absorption of phosphate by the kidneys."

FREDERICK S. BACON, director of a laboratory for chemical research and consultation in Watertown, Mass., has been elected chairman of the Northeastern Section of the American Chemical Society.

THE following officers were elected at the annual general meeting of the British Rheologists' Club, held on November 6: *President,* Dr. C. F. Goodeve; *Honorary Secretary,* Dr. G. W. Scott Blair; *Honorary Treasurer,* Dr. V. G. W. Harrison. After the transaction of formal business, Professor E. H. Rideal gave an informal address on "Solutions of Macromolecules."

DR. CYRIL BANKS, of Nottingham, has been installed as president of the British Society of Medical Officers of Health.

PROFESSOR EDWARD M. LEHNERTS, chairman of the department of geology and geography of Hunter College, New York City, has retired. He was appointed associate professor in the department of natural sciences in 1920, and became head of the department of geology when it was established in 1921. In 1929, he was promoted to a full professorship and in 1938 became chairman of the department of geology. Professor Henry D. Thompson has been elected to finish the unexpired term of Professor Lehnerts.

THE *Journal* of the American Medical Association states that Dr. Joseph S. Lichty, Shaker Heights, Ohio, of the Cleveland Trust Company, has been appointed assistant dean of the faculty of medicine at Harvard University and assistant professor of medical administration. Dr. Franklin F. Snyder, Chicago, has been appointed associate professor of anatomy and obstetrics at the Medical School. Dr. Frederick J. Stare, of the University of Wisconsin, has been appointed assistant professor of nutrition, which is reported to be a new title connecting the clinical and laboratory branches of nutritional science. Dr. Stare will work both in the laboratory and in the field in connection with the Medical School and the School of Public Health. Dr. Henry P. Treffers, an immunochemist, has been named to the newly established position of assistant professor of comparative pathology and biologic chemistry. A new office, known as "Medical School Research Laboratories," has been organized under U. Haskell Crocker. The object is to coordinate and centralize the business details of the various research projects in progress, operating under contract with the Office of Scientific Research and Development.

DR. WALTER B. CARVER, professor of mathematics at Cornell University, takes office on January 1 as secretary-treasurer of the Mathematical Association of America. The national headquarters of the association will be moved to Cornell University from Oberlin College. Professor Carver succeeds Professor W. D. Cairns, of Oberlin, who retires from the position he has held since the association was founded in 1915.

DR. SIDNEY J. FRENCH, professor of chemistry at Colgate University, has been appointed to serve as coordinator of the Naval Flight Preparatory School which will open on January 7. He will organize and direct the new faculty, which will include about fifty teachers. An attendance of six hundred aviation cadets is expected.

DR. CHESTER NORTH FRAZIER has been made professor of dermatology and syphilology at the Medical School of the University of Texas at Galveston. He was formerly professor of dermatology and syphilology at Peiping Union Medical College, China, where he had served for some twenty years. While at Peiping he was instrumental in helping to develop the medical library of the college.

DR. M. DEMEREC, who has been serving as acting

director of the Department of Genetics of the Carnegie Institution of Washington at Cold Spring Harbor, L. I., N. Y., becomes director on January 1, and Dr. Thorne M. Carpenter, acting director, has been made director of the Nutrition Laboratory of the institution in Boston. Dr. Demerec has been associated with the Department of Genetics since 1923, and Dr. Carpenter with the Nutrition Laboratory since 1907.

DR. JOHN W. HEUBERGER, plant pathologist at the Connecticut Agricultural Experiment Station, New Haven, left at the end of November to join the research staff of the Rohm and Haas Company of Philadelphia. Working under the Crop Protection Institute at the New Haven Station, Dr. Heuberger carried on research in organic fungicides. He will continue this work at the Subtropical Experiment Station, Homestead, Fla.

DR. ROBERT L. PENDLETON has been appointed principal soil technologist at the Office of Foreign Agricultural Relations of the U. S. Department of Agriculture. He has been detailed to proceed to Nicaragua and other countries in tropical Latin America in connection with the establishment of the joint agricultural experiment stations. Until Thailand declared war on the United States, he was agricultural adviser and soil scientist to the government there. He was thereafter interned and repatriated on the *Gripsholm*.

PROFESSOR EDWARD KASNER, of Columbia University, recently opened a series of lectures at St. John's College, Annapolis, on the relations of mathematics to the sciences.

ACCORDING to an Associated Press dispatch, Dr. Theodore Dykstra, of the Bureau of Plant Breeding of the U. S. Department of Agriculture, and Dr. Walter C. Lowdermilk, chief of the Division of Research in Soil Conservation, arrived in Chungking on December 10. They left the United States on September 15. They will be joined by a corps of approximately thirty American specialists who will assist in the modernization of the agricultural methods of China.

DR. DAVID RANDALL PYE, formerly fellow of Trinity College, Cambridge, and of New College, Oxford, who has been for many years connected with the scientific side of aeronautics and who succeeded H. E. Wimperis as director of scientific research to the British Air Ministry, has been appointed to succeed Sir Allen Mawer as provost of University College, London.

WILLIAM B. WIEGAND, of the Columbian Carbon Company of New York, has been appointed a member of the Synthetic Rubber Advisory Committee of the Department of Munitions, Ottawa, Canada. Dr. R. V. Yohe, of Akron, Ohio, has retired from the committee.

DR. HENRY B. BIGELOW, professor of zoology and curator of oceanography at Harvard University, has been appointed special representative in charge of Navy V–1 and V–7 enlistments at the university.

GEORGE A. HARDER, publicity director of the Westinghouse Radio Stations, Inc., has reported for active service as a captain in the chemical warfare branch of the Army.

SCIENCE has received the following correction from the Materiel Center of the Army Air Forces: "Attention is directed to the first paragraph of the article 'Oxygen Masks for the A.A.F.' on page 531, of the December 11, 1942, issue of SCIENCE, where a misstatement of fact has been made. The Field Museum of Chicago, Illinois, has not been invited to supervise the production of oxygen masks for the Army Air Forces nor have they been consulted at any time by the Materiel Center in the design of these masks."

THE American Mathematical Society met at the University of Notre Dame on November 27 and 28, with an attendance of seventy mathematicians, eight states being represented. Twelve research papers were presented and discussed and eight additional papers were read by title. As a part of the meeting, the University of Notre Dame held its annual Mathematical Symposium, the subject being "Modern Statistics." In this symposium Professors Jerzy Neyman, of the University of California, and Abraham Wald, of Columbia University, gave two lectures each covering their recent work on the statistical theory.

THE History of Science Society announces the cancellation of its December meeting. The annual business meeting of the society will be held on Thursday, January 7, at 3 P.M., at the Hotel Pennsylvania, New York City.

PRESIDENT ROOSEVELT has signed a bill authorizing the rank of rear admiral in the Dental Corps of the Navy. Previously the highest rank in that corps was captain. It is explained that the Dental Corps is expanding rapidly and that the senior position there now should receive a rank comparable to that of a chief Navy physician, which is rear admiral.

FOR accomplishing "more than seemed reasonable or possible" in the production of chemicals and metals for war, the Dow Chemical Company was presented, on December 29, with two Army-Navy "E" pennants. The Dow Chemical Company is one of the largest producers of industrial chemicals and pioneered in the production of magnesium metal. The chief speaker at the presentation was Major General William N. Porter, chief of the Chemical Warfare Service.

AT a recent general meeting of the Zoological So-

ciety of London, the council, according to the *Times*, London, reported that the number of visitors to the Gardens during the past three months was 629,227 and receipts for admission amounted to £30,091. The total number of visitors during the year up to the end of October was 1,313,821 and the receipts amounted to £56,030, showing an increase of £39,504 compared with the corresponding period of the previous year and of £14,203 compared with the average for the previous five years. The number of visitors to Whipsnade Park during the year up to the end of October was 99,510 and receipts amounted to £4,581, showing a decrease of £5,322 compared with the corresponding period of the previous year.

DISCUSSION

SEVERITY OF NARCISSUS BASAL ROT INCREASED BY THE USE OF SYNTHETIC HORMONES AND NITROGEN BASES

COMMERCIAL bulb growers have recently become interested in the possibility of using growth-regulating substances to obtain increased flower and bulb production as well as disease control. Several materials purporting to accomplish these objectives are now on the market. The effect of a number of such compounds on narcissus bulbs, variety King Alfred, has been studied at the Bureau of Plant Industry Station, Beltsville, Maryland, during the past two years. Included in these studies have been naphthalene acetamide, indolebutyric acid, indoleacetic acid, naphthaleneacetic acid, uric acid, guanidine and allantoin. Before planting in the fall some bulbs were dipped in solutions containing 10 to 100 p.p.m. of these compounds, others in talcum powder containing 1 to 10 parts of these compounds in 5,000 parts of talcum. Several thousand bulbs were planted in suitably replicated plots. In every trial the application of the hormones or nitrogen bases increased the amount of basal rot over that in comparable untreated bulbs. Bulbs apparently healthy were treated after harvest with several of the above-named compounds and developed a significant increase in the amount of basal rot during storage.

In laboratory studies the addition of naphthalene acetamide, indolebutyric acid, allantoin, uric acid and adenine sulfate stimulated the growth of *Fusarium oxysporum* f. *narcissi* (Cke. et Mass.) Sny. and Han., the causal organism of basal rot. This is believed to be the first report of stimulation of a plant pathogen by a synthetic growth-regulating substance of the hormone type or by a nitrogen base. Data presented in a paper by Greathouse and Rigler[1] seem to indicate that increased growth of *Phymatotrichum omnivorum* (Shear) Duggar occurred when xanthine and adrenaline were added to the nutrient solution. At the Buffalo, New York, meeting of the American Chemical Society in 1942 Martin and Fisher reported that adenine increased the virulence of *Escherichia coli*.[2]

[1] Glenn A. Greathouse and Neil E. Rigler, *Phytopath.*, 30: 475–485. 1940.

[2] Gustav J. Martin and C. Virginia Fisher, *Abstracts*, American Chemical Society meetings, 28B, 1942.

Further studies on the effect of synthetic hormones and nitrogen bases on growth and pathogenicity of the basal-rot pathogen and other organisms are in progress and a more extensive report will be presented elsewhere.

NEIL W. STUART
W. D. MCCLELLAN

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WHAT IS A GENERIC NAME?

RECENT discussion in this journal on the use of generic names has surprised and puzzled some of my colleagues and me. Certain scientists appear to be exercising a remarkable ingenuity in extricating themselves from a difficulty which does not exist. To these persons it seems to violate some sacred canon to add a plural (or other) termination to the Latin name of a genus. *Paramecium* is correct, but I may not write *Paramecia;* instead I must struggle with such unwieldy phrases as "specimens of *Paramecium*." Or, to adopt the proposed solution of the "difficulty," I may transform the name into a "common noun" and write "paramecia."

According to S. O. Mast,[1] "a generic name is always a collective noun" and "refers to all the individuals which are similar to the type specimens of the genus." By grammatical definition, therefore, it is not a proper noun, which is the name of a single individual. But the same writer further implies that it is not a common noun, since he says he has "obviated this [the difficulty above mentioned] by using the generic name as a common noun." If a name is neither a common noun nor a proper noun, would it be improper to refer to it as an uncommon noun? C. D. Beers,[2] however, gives us the clue to this confusion when he remarks: "The following are some common animal names (hence common nouns). . . ." By "common nouns" in this discussion we are evidently to understand words in everyday use, and nothing grammatical.

The trouble is deeper than grammar—and more important. "A generic name . . . is always in the

[1] SCIENCE, 96, 252, 1942.

[2] SCIENCE, 96, 403, 404, 1942.

nominative case and it is always singular in number." I believe that Mast is here quoting or at least paraphrasing the rules of zoological nomenclature. The corresponding botanical code says that "Names of genera are substantives . . . in the singular number . . ." without allusion to their case. The difficulties adumbrated by the authors cited originate in a too servile adherence to these carelessly worded dicta. The botanical version has one solecism fewer than the zoological, but it is inconceivable that either represents the real thought of the framers of the codes. Obviously, the name of a genus is a nominative singular when taken by itself, or for entry in a catalogue or index; likewise the name of a family is a nominative plural. It can scarcely be the sense of any self-respecting code of nomenclature that such a name can not be treated as other names are treated when it is introduced into discourse. When a generic name is the object of a verb, it is no longer "in the nominative case"—rules or no rules.

Linnaeus wrote: "Cerealia sunt semina majora graminum . . .: *Oryza, Triticum, . . . Mays,* excepto forte solo *Lolio,* nisi arte praeparato." And again: "Semina minora *Phalaridis, Panici, Milii, . . .*" More than a hundred years later Bentham and Hooker wrote: "Genus potius *Sisyrinchio* quam *Solenomali* affine videtur." A modern writer, describing a new genus of algae, characterizes its thallus as "erectus ex fundamento radicato in cryptostomatibus *Cystoseirae* immerso." How else could you say these things? To say that a word is "always in the nominative singular" is tantamount to saying that it can not be used in a sentence except as the subject of the verb. Are we to pretend that the italicized words in these quotations are not names of genera but "common nouns"? In English we have no endings for genitives and ablatives; so we say "seeds of *Panicum*" and the like. But we do have plural forms, and we need not hesitate to use in an English sentence the plurals of Latin words, as we do those of *nucleus* and *alumnus. Crataegi* means members of the genus *Crataegus,* as "the Smiths" means members of the Smith family. If English had case-inflections, we should undoubtedly enjoy dative, ablative, locative and genitive Smiths, to say nothing of vocative Smiths ("O Smittee . . .").

Obviously, there is need here for clarification of the rules of nomenclature. To say that names can not be declined is not only without precedent in grammar or in science—it is without use. If our steed is to carry us surely and swiftly, it is inadvisable to hamstring him. What mirth would be provoked among the "fathers" of our science if they could see their successors laboring to render impotent the technical language which they devised!

H. W. RICKETT
THE NEW YORK BOTANICAL GARDEN

A NEW GENERAL TERM FOR MINERAL INDUSTRIES STUDIES

DURING the recent summer the undersigned received from Dean Edward Steidle, of The Pennsylvania State College School of Mineral Industries, a letter part of which follows:

> I am trying to find a word that will be all-embracive for earth sciences, mineral economics, mineral engineering and mineral technology, i.e. mineral service, mineral work or mineral utilization. If there is no word, I have in mind that a new word might be coined. . . .

The purpose of the present communication is to bring before earth scientists the term that the undersigned has suggested. It appears to embrace the techniques and studies involved, to be readily pronounced, to be easily recognized and understood, and to the writer, a Hellenist, to have the virtue of sound etymological formation. The new word is "geotechnology."

It is the considered opinion of the writer and of Dean Steidle that this is a new term, and we thus record it. Specific reference to contrary evidence will be greatly appreciated.

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SPECIAL CORRESPONDENCE

SUGGESTIONS FROM THE OFFICE OF SCIENTIFIC PERSONNEL OF THE NATIONAL RESEARCH COUNCIL

As soon as the Army and Navy training programs are in full operation there will be an unprecedented demand for teachers of physics and mathematics. The situation will be particularly critical in the field of physics where the teaching ranks of colleges and universities have already been seriously depleted.

It is the business of the Office of Scientific Personnel to assist in the placing of the scientific specialist where he can best serve the war effort. Because the present supply of physicists approximates zero and the supply of mathematicians is running low, perhaps this office can assist best by suggesting two sources of supply close at hand to the institutions which are so fortunate as to secure Army and Navy contracts.

The first source of supply is the near-by institutions which will not have Army or Navy training programs. Although these institutions should con-

tinue to teach physics to as many students as possible, some departments of physics will find it impossible to continue in operation and their staff members should be added to departments in need of their services.

The reason why all departments of physics which can possibly do so should continue in operation is that it is probable that the needs of the Army and the Navy will be barely satisfied through the training programs. The needs of war research, war industry and teaching must be satisfied, for the most part, by women and the physically unfit. In this connection it should be mentioned that there seems to be no possibility of meeting the need for competent teachers of physics in the secondary schools.

Accordingly, it is important that colleges which are not fortunate enough to secure training contracts should continue to teach physics to even larger numbers than before. So great is the need for men and women trained in physics that every effort should be made to recruit into departments of physics all students with the necessary aptitude.

These considerations make it clear that the more favored departments should not take men from other institutions unless it is necessary for these men to find employment elsewhere. The other source of supply, and the one that should be utilized wherever possible, is within the institution itself.

In any college or university there are teachers in other fields, including botany, geology, physiology, psychology and zoology, who have sufficient knowledge of physics so that, with a little brushing up and some observation of good physics teachers at work, they should become proficient teachers of beginning physics. These men and women should be found at once and encouraged to prepare themselves for the teaching service which they will almost certainly be called upon to perform either at their own institutions or elsewhere.

Similar adjustments, although on a smaller scale, will be necessary in the field of mathematics.

It is the hope of this office that most of the required readjustments in the staffs of physics and mathematics departments may be worked out by drawing in new staff members from near-by institutions or by adjustments within the institutions themselves. This office will facilitate such adjustments in every way possible if they can not be worked out locally and will be glad to receive information regarding institutional needs and available personnel but only with the understanding that we will attempt to prevent bidding of institution against institution in mad competition for personnel. It is to the interest of all concerned that a sufficient supply of teachers of physics and mathematics should be developed so that the necessary readjustments may be made easily and efficiently.

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SCIENTIFIC BOOKS

ADVANCES IN ENZYMOLOGY

Advances in Enzymology. Volume II. Edited by F. F. NORD and C. H. WERKMAN. 374 pp. Interscience Publishers Inc., New York. 1942.

THE second volume of "Advances in Enzymology," this excellent successor to "Ergebnisse für Enzymforschung," edited by Nord and Werkman, has fulfilled the expectations of those who read the first volume, although there are some articles which have departed somewhat from the field a book of this kind is expected to cover.

The first article in particular, "Bacterial Viruses (Bacteriophages)" by M. Delbrück, seems to be outside the scope of enzymology. After calling bacteriophage a virus ("what's in a name?") Delbrück tells his readers that there are at present two aspects of the phage problem of particular interest: "the biochemical basis of the specific relation of the phage to its host" and "the problem of phage growth." Then he proceeds with remarks about "distribution in nature," "methods of assay," "the 'Life Cycle' of virus in sensitive hosts," etc., and leaves the reader with the same curiosity as when he started regarding the two aspects of particular interest. Neither of the two aspects is discussed in a manner to satisfy or clarify the mind of the student of enzymes.

The kinetics of hydrolytic enzymes has been presented by D. D. Van Slyke in the second article with his usual clarity. The assumption of substrate-enzyme complex formation first formulated by Michaelis and now generally accepted has been treated by Van Slyke with such unusual success that a study of this paper is recommended to any one interested in the kinetics of enzyme systems in general. The "Classification of Proteolytic Enzymes" by M. Bergman fills a need long felt. Professor Bergman must be congratulated for having resolutely discarded the outmoded but tenaciously retained classification of proteolytic enzymes in peptidases and proteinases. His tentative classification, based on two characteristics of proteolytic specificity, namely, that each proteolytic enzyme

requires the presence of certain atomic groupings within the main body and within the side chain of the reacting molecule, is a step forward toward a rational nomenclature. Bergman's discussion of the nature of proteolytic enzymes is evidence that the rapid progress made in the field of oxidation enzymes has also aided in the better understanding of the nature of other enzymes. Proteolytic enzymes are far simpler than oxidation enzymes as they resemble in their chemical constitution the first type of oxidation enzymes, the metalloproteins; the fact that certain metals act as "activators" does not mean, as Bergman correctly points out, that "the active enzyme is composed of an inactive apoenzyme and an activator" but that the particular enzyme is made up of a protein combined to a metal both being equally essential for enzyme action in the same manner as polyphenol oxidase is a Cu-protein. The example of coupled reaction given by Bergman—the hydrolysis of glycyl-l-leucine by papain on addition of acetyl-phenylalanylglycine—is so clearly presented that the reader can immediately understand the observations and conclusions of Schoenheimer on the continuous splitting and synthesis of the protein molecule wherever those enzymes are present. Johnson and Berger provide, in their article on the enzymatic properties of peptidases, more examples of proteolytic enzymes made up of metalloproteins (Zn, Mn, Mg-proteins).

Zeller's article on diamine oxidase shows how difficult it is to discard old, misleading terminologies (aerodehydrases, oxydehydrases). In spite of the considerable work done by Zeller and his coworkers, in spite of the solubility of the enzyme, it has not yet been prepared in reasonably purified condition; furthermore, the enzyme preparation of Zeller, the properties of which are discussed in this paper, is rather inactive compared with that of Laskowski (personal communication). The problem of specificity, of the components of the enzyme system, will not be elucidated until a better preparation of diamineoxidase than that of Zeller is obtained. His excellent contributions will be always helpful. Roberts, who for years has studied the chemical processes during the manufacture of black tea, discusses the chemistry of tea-fermentation (oxidation of the leaf-tannins). The author's hypothesis that tea-tannins are oxidized by cytochrome oxidase has no valid support. Although we are in thorough agreement with Roberts in his insistence that cytochrome oxidase might be different in different cells, and would readily grant that cytochrome oxidase exists in tea-leaf, we must conclude that he offers no evidence for his opinion. No argument is given against the simpler assumption that the enzyme is a Cu-protein similar to polyphenol oxidase. Tea tannins consist mainly of the simpler condensation products of epi-catechin and gallo-catechin, o-quinones being the first product of oxidation; these, according to Roberts, condense afterwards. The kinetics of these condensation reactions are briefly discussed. Roberts's discussion of the mechanism of tea-fermentation must be read with attention by all students of oxidation-reduction processes. Although his conclusions and his hypotheses have no factual foundation and even run counter to established concepts, the presentation of the problem opens a field full of questions to be answered.

To discuss the problem of heterotrophic assimilation of carbon dioxide no more appropriate selection could have been made than that of Werkman and Wood, the undisputed pioneers and leaders in this field, the investigators who for the last seven years have been courageously trying to convince biologists that living cells could assimilate CO_2 by other processes than photosynthesis. This chapter is so well presented that the reviewer can only recommend its thorough study to all biologists interested in the physico-chemical processes of life.

H. Tamiya, who with his coworkers has contributed greatly to our knowledge of mould metabolism, has written the chapter on "Respiration, Fermentation and Enzyme Systems of Aspergillus." The exhaustive and careful study of the total metabolic balance of this mould, so useful to Japan, unfortunately has not been followed by a study of the mechanisms of oxidation-reduction. The pathways of carbohydrate fermentation and oxidation, for example, remain still a subject for study. It is especially unfortunate because *Aspergillus* seems to be one of those moulds where non-phosphorylating glycolysis exists, where the Szent-Györgyi cycle of accessory oxidation catalysts does not operate.

In their article on cellulose decomposition by microorganisms, Norman and Fuller give, after a brief presentation of the chemical structure and properties of cellulose, a good presentation of the probable mechanism of cellulose decomposition by bacteria and fungi. The extreme abundance of cellulose makes a careful study of these probable mechanisms necessary, because the controlled hydrolysis of cellulose or the controlled oxidation of its hydrolytic products might be used for the production on a large scale of the products of hydrolysis or oxidation.

"A Unified Hypothesis of the Reciprocal Integration of Carbohydrate and Fat Catabolism" is the title of an essay by E. J. Witzemann. The twenty "fundamental facts relating to fat and carbohydrate catabolism" do not help the reader in understanding the "unified hypothesis." Moreover, the first three of these fundamental facts are so naïve that the reader is left with no desire to go on with the remaining seventeen. The role assigned to choline in fat metabo-

lism is not in agreement with McHenry's conception of the function of choline, nor has it factual support. Biologists now go farther than Witzemann; they are aware of the interrelation not only of fat and carbohydrate metabolism but of all foodstuffs. They have been speaking for some time of pyruvic acid as the hub towards which the breakdown of foodstuff (carbohydrate, fat, protein) converges, from which synthesis starts.

H. Dam, the discoverer of vitamin K, has given us an excellent review of the chemical and physiological properties of this vitamin, and has shown that there still remain many obscure problems, such as the nature of the action of vitamin K on prothrombin formation.

Pfiffner's article on the "Adrenal Cortical Hormones" concludes the book. After reviewing the chemical properties of the different steroids extracted from the adrenals, the methods of assay, Pfiffner devotes one page (out of 27) to the effect of corticosterones on carbohydrate metabolism, unfortunate neglect when the article is written for "Advances in Enzymology."

The publishers are to be congratulated for the excellent care with which the book has been presented. The errors found in Van Slyke's article (pp. 34 and 39) were promptly corrected. It is unnecessary to say that this series must be in the library of every laboratory where there is interest in the mechanism of biochemical activities.

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BIOLOGICAL SYMPOSIA

Comparative Biochemistry, Intermediate Metabolism of Fats, Carbohydrate Metabolism and the Biochemistry of Choline. Edited by HOWARD B. LEWIS. ix + 247 pp. Illustrated. Jaques Cattell Press. 1942.

THE fifth volume of "Biological Symposia" deals with comparative biochemistry, intermediate metabolism of fats, carbohydrate metabolism and the biochemistry of choline. This volume is edited by Professor Howard B. Lewis, who provides a short introduction for it.

It may be stated immediately that there is a wealth of information in this collection of scientific articles. Those in which comparative biochemistry is discussed will be found of great value, not only to students who are interested in this subject specifically, but to workers in all branches of the biological sciences. The article on the end products of nitrogen metabolism in plants will be stimulating to those who are interested in nitrogen metabolism of animals. This latter subject is also discussed and it may in turn suggest new approaches to the problems of the plant biochemists and physiologists.

The article on the merging of growth factors and vitamins proves again the point that investigators of bacterial metabolism must keep pace with the rapid growth of our knowledge of the vitamins, while the vitamin experts will profit greatly by studying the results of investigations of the metabolism of bacteria.

Four articles on the intermediate metabolism of fat help us to keep abreast of this rapidly growing field and the changing views which must be adopted in light of the accumulating evidence. The symposium on carbohydrate metabolism deals primarily with the more purely biochemical aspects of the study and consists of four stimulating articles on "Oxidation Catalysts," "Phosphorylation of Glycogen and Glucose," "Oxidoreduction in Carbohydrate Breakdown" and "Pyruvate Oxidation and the Citric Acid Cycle." There is little doubt that many of the fundamental changes in sugars within the body are now being revealed. These short reviews should help materially to bring the subject up to date. Here again the interrelationship of the various fields discussed in this volume is obvious. The accessory food factors are assuming an ever-increasing role in all considerations of protein, fat and carbohydrate metabolism.

The four articles on choline provide a most useful picture of this relatively new but rapidly extending field. Choline, as a dietary factor, is now known to be intimately related to fat, protein, and more indirectly to carbohydrate metabolism as well as to many of the components of the vitamin B complex.

In this review it will be impossible, of course, to make any detailed summary of the information found in any of the four divisions into which the symposia fall. The general impression given by the whole volume is that a great deal of essential information has been gathered together and presented in a most pleasing and stimulating way. One is left with the feeling that a much more extended review of each of the fields would be most acceptable. This, however, would not be possible under the conditions of presentation of these symposia. It is to be hoped that editors will be found and the necessary arrangements made for the publication of the symposia which have more recently been presented before the Federation of American Societies for Experimental Biology.

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HURRICANES

Hurricanes. By I. R. TANNEHILL. 2nd edition, 8vo, x + 256 pages. 119 figures. Princeton University Press. 1942.

THIS is one of the few meteorological "must" books. Teachers, at least, of meteorology and climatology must keep it close by for reliable reference. Mr. Tannehill, as chief of the Marine Division of the U. S. Weather Bureau, needed to have as full information as practicable of tropical storms in general and of the West Indies hurricane in particular, a need which his official position and the facilities of the Bureau's extensive library enabled him to meet. This information he neither lazily nor selfishly put aside for his private use, but industriously got together for the convenience of the hundreds of others who might want it.

It is a readable book, full of facts, historical and otherwise; and free from tedious theoretical discussions that would have had to be based at best on doubtful assumptions. Logically it begins with a clear description of a tropical cyclone or hurricane as one in the general region of the West Indies is called; of how reports of such a storm are obtained, and how warnings of its approach are issued.

This general discussion is followed by a number of chapters that consider in detail the winds, the barometric pressure, the torrential rains, tidal waves and other phenomena that accompany or are parts and parcels of a typical hurricane. The tracks of many of these storms are given, and their frequency, both annual and monthly, noted.

The last four chapters give the history of all West Indian hurricanes of record, extending from 1493 to 1941. One of these storms wrecked, as history tells us, certain vessels on their way, in 1609, to Jamestown, and inspired Shakespeare to write "The Tempest."

The book contains also a very full and, for students of the subject, an invaluable bibliography of hurricane literature and a good index.

W. J. HUMPHREYS

SPECIAL ARTICLES

COMPLEMENT FIXATION IN RICKETTSIAL DISEASES

THE complement fixation technique has proved adequate for serological separation of the rickettsial agents of endemic typhus,[1, 2] "Q" fever[3] and Rocky Mountain spotted fever.[4] The agents of epidemic and endemic typhus previously have not been distinguishable by this method, since cross reactions were obtained with materials from the two closely related diseases.[2, 5] It is now possible to prepare rickettsial antigens with which we can differentiate epidemic and endemic typhus antisera. Certain of the results obtained with these new antigens are of sufficient clinical and epidemiological interest to record at this time; the details of the method of preparation of the antigens will be given at a future date.

The method employed in performing the complement fixation test has been described.[4] The epidemic and endemic rickettsial antigens (Breinl and Wilmington strains) are prepared from infected yolk sacs of developing chick eggs and purified so that the diluted antigens contained 0.02 mg N per cc. The epidemic and endemic rickettsial antigens are standardized against convalescent sera of known titre obtained from human beings and guinea pigs recovered from epidemic and endemic typhus. Some cross fixation occasionally occurs, but when it does, the specific antigen always reacted with the homologous serum in a higher titre than with the heterologous serum.

Table I indicates the results obtained on specimens of serum obtained during convalescence in epidemic and endemic typhus and in Brill's disease.

In 43 cases of endemic typhus fever examined 36 gave a positive complement fixation with an endemic rickettsial antigen and a negative fixation with an epidemic rickettsial antigen. In six cases there was some cross fixation, but in every case where this occurred, the titre obtained with the endemic antigen exceeded that obtained with an epidemic antigen.

In 29 cases of epidemic typhus fever 26 gave a positive fixation with an epidemic rickettsial antigen and a negative test with an endemic antigen. In three cases there was some cross fixation, but in all instances where this occurred the titre obtained with the epidemic antigen exceeded that obtained with an endemic antigen.

All the patients included in the group of endemic typhus come from one of two areas in which this type of infection is known to occur, but in which epidemic typhus has not been found. The patients comprising the group of epidemic typhus come, in part, from an area where louse-borne typhus exists and, in addition, individuals are included who had laboratory infections and from whom epidemic strains have been isolated.

It has been observed that the immigrants who contract Brill's disease usually give a history of having had typhus while in Poland or Russia. Only one case

[1] M. R. Castaneda, *Jour. Immunology*, 31: 285–291, 1936.
[2] I. A. Bengtson, *Pub. Health Rep.*, 56: 649–653, 1941.
[3] I. A. Bengtson, *Proc. Soc. Exp. Biol. and Med.*, 46: 665–668, 1941.
[4] H. Plotz and K. Wertman, SCIENCE, 93: 441–442, 1942.
[5] Unpublished data.

TABLE 1

	No. of cases	Epidemic Rickettsial Antigen	Dil. of serum	Endemic Rickettsial Antigen	Dil. of serum
Endemic Typhus					
43 cases	5	negative		4 plus	1 : 12
	12	negative		4 plus	1 : 24
	6	negative		4 plus	1 : 48
	9	negative		4 plus	1 : 96
	2	negative		4 plus	1 : 192
	1	4 plus	1 : 6	4 plus	1 : 96
	1	4 plus	1 : 6	4 plus	1 : 192
	4	4 plus	1 : 12	4 plus	1 : 48
	2	4 plus	1 : 12	4 plus	1 : 96
	1	4 plus	1 : 24	4 plus	1 : 384
Epidemic Typhus					
29 cases	2	4 plus	1 : 12	negative	
	1	4 plus	1 : 24	negative	
	1	4 plus	1 : 48	negative	
	2	4 plus	1 : 96	negative	
	13	4 plus	1 : 192	negative	
	6	4 plus	1 : 384	negative	
	1	4 plus	1 : 768	negative	
	1	4 plus	1 : 96	4 plus	1 : 24
	1	4 plus	1 : 192	4 plus	1 : 48
	1	4 plus	1 : 192	4 plus	1 : 96
Brill's disease					
23 cases	4	4 plus	1 : 12	negative	
	2	4 plus	1 : 24	negative	
	4	4 plus	1 : 48	negative	
	2	4 plus	1 : 48	4 plus	1 : 6
	1	4 plus	1 : 48	4 plus	1 : 12
	1	4 plus	1 : 96	4 plus	1 : 48
	1	4 plus	1 : 96	4 plus	1 : 12
	2	4 plus	1 : 384	4 plus	1 : 48
	3	4 plus	1 : 768	4 plus	1 : 192
	1	4 plus	1 : 960	4 plus	1 : 96
	1	4 plus	1 : 960	4 plus	1 : 384
	1	4 plus	1 : 1436	4 plus	1 : 192

usually occurs in a family, no vector or reservoir of the virus has been found, and the virus isolated from the blood of the patient has the characteristics observed for the epidemic strain. On the basis of these observations Zinsser[6] advanced the theory that Brill's disease represented a recrudescence of an old attack of typhus fever. Our observations bring serological evidence to substantiate this point of view.

In 23 cases of Brill's disease examined all showed a positive complement fixation with an epidemic rickettsial antigen. In 10 cases there was fixation with an epidemic rickettsial antigen and no fixation with an endemic rickettsial antigen. In 13 cases there was some cross fixation but in all instances where this occurred the titre obtained was higher with an epidemic antigen. The pattern of fixation in this disease resembles that obtained in epidemic typhus fever.

Absorption tests were performed on specimens of serum from Brill's disease where cross fixation had occurred. An endemic rickettsial antigen removed all the endemic antibody with slight effect upon the titre of epidemic antibody. On the other hand, a similar treatment of the serum with an epidemic rickettsial antigen resulted in the removal of both the epidemic and endemic antibody; no selectivity of absorption was observed. These results would indicate that the endemic rickettsial antigen pattern was different from that of the antigenic pattern of the epidemic strain. The removal unselectively of both endemic and epidemic antibodies by the epidemic antigen suggests that the epidemic antigen may be a more complete or complex antigen than the endemic antigen.

The implication of the results obtained in Brill's disease on the epidemiology of typhus fever is great. They would indicate that mild cases of epidemic typhus actually exist in the United States. The disease is not transmitted from person to person in this country simply because the louse vector is not present. Furthermore, these results indicate that one attack of typhus does not confer a lifelong immunity as is generally believed. The virus is probably harbored in the body and when the resistance is lowered the virus multiplies and induces a mild attack of the disease. If these cases should occur in a louse-infested community the disease might readily spread from person to person. The observations on Brill's disease strongly suggest that man serves as the reservoir for epidemic typhus between outbreaks just as the rat does in endemic typhus.

The complement fixation test now provides a tool with which surveys of the prevailing types of typhus in a region can be determined. This procedure has been applied and endemic typhus has been discovered in Jamaica and epidemic typhus in a South American country. These surveys are now being continued in other countries.

The author wishes to thank Captain S. C. Bukantz, Lt. K. Wertman, Lt. L. Pillemer and Miss N. Rogers for their assistance, as well as Dr. E. Schoenbach for having provided the specimens of serum from cases of Brill's disease.

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[6] H. Zinsser, *Am. Jour. Hyg.*, 20: 513, 1934.

A STUDY OF CRYSTALLINE BEEF LIVER CATALASE DRIED IN THE FROZEN STATE[1]

INTRODUCTION

THE drying of proteins by the lyophile process is assuming increasing importance in research and in preparing such material as dried plasma or dried pollen extracts for practical use. There is a tendency to assume that in drying a quickly frozen protein preparation by subjecting it to a high vacuum, no change such as denaturation occurs. The following observations show that this is not necessarily true.

[1] From the Department of Biochemistry and Pharmacology and the Department of Medicine, The University of Rochester School of Medicine and Dentistry, Rochester, New York.

They also indicate that lyophilization might be used in selected cases as a means to study the process of protein denaturation. Finally they make it appear possible that new information about the chemistry of an enzyme molecule may be obtained from a study of the lyophilized material.

EXPERIMENTAL

Twice-crystallized beef liver catalase,[2] consisting of a suspension of plates and prisms in water, was quickly frozen in a thin layer by cooling in an acetone-dry ice bath. Then a vacuum pump was attached and the preparation was removed from the cold bath and left connected with the pump until drying was complete. The frozen suspension did not melt at any time during the drying. A separate sample that was heated in the oven to 100° C for two hours did not change in weight.

The dried powder was suspended in water and was examined microscopically. Large crystals of the prism type were disintegrated, but the small crystals of this type retained their original form. The plates were fragmented. The dried preparation though insoluble in water was easily soluble in excess M/1,000 disodium phosphate. In concentrated suspensions it became soluble if sufficient disodium phosphate was added to bring the pH to 8.0. By this procedure, concentrated solutions were obtained which showed a slight opalescence and gave a very strong qualitative test for catalase. The usual three-banded catalase spectrum was present. Quantitative determinations however showed that the activity per dry weight of the material was only about one third that of undried crystalline beef liver catalase,[2] since the *Kat. f.* was about 11,000.

The solubility of the dried preparation differs from that of the undried material, since it is only slightly soluble below pH 8 in the presence of 10 per cent. NaCl. Moreover, it has not been possible to crystallize the dried material, either by the ammonium sulfate method[3] or by the dialysis method.[2] This indicates that a change has been brought about in the enzyme molecule, such as a mild denaturation. It is of interest in this connection that lyophilized catalase keeps quite well in a desiccator, but if left exposed to the atmosphere it gradually loses its solubility during a period of about two months and finally becomes quite insoluble and inactive.

The absorption spectrum of dried catalase was compared with that of undried catalase by Mr. Ralph Brauer of this Department of Biochemistry and Pharmacology, using a visual spectrophotometer. The two samples were adjusted in concentration so that the intensities of the bands at 630 mμ were equal. We have not yet determined whether following this procedure the concentrations on a dry-weight basis are equal. The two absorption spectra do not appear to differ significantly, as shown in Fig. I.

Dried catalase is inhibited by NH_2OH and HCN, as is undried catalase. In the presence of NH_2OH, the addition of a little hydrogen peroxide causes the appearance of the two-banded spectrum reported by Keilin and Hartree[4] for undried catalase. Dried

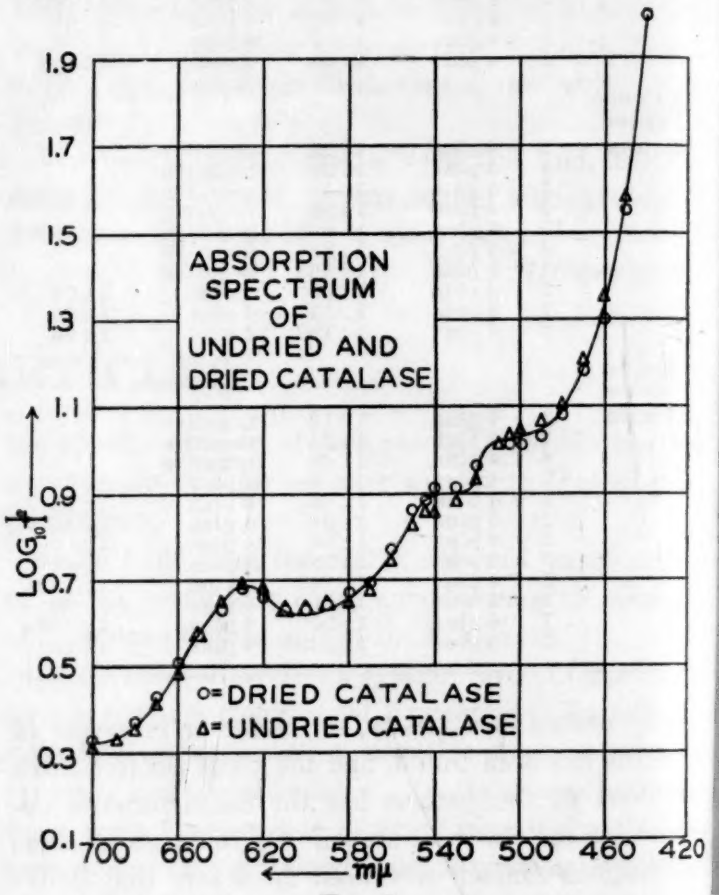

FIG. 1

catalase gives the same bands with HCN, as does undried. Thus the behavior of the dried catalase towards NH_2OH and HCN seems to be identical with that of undried catalase.

One of the peculiarities of ordinary undried catalase is that the hematin iron is not reduced by $Na_2S_2O_4$ alone. However, Zeile *et al.*[5] reported that they were able to reduce the catalase-H_2S compound with $Na_2S_2O_4$ after excess hydrogen sulfide had been removed by passing an inert gas through the solution. If oxygen were admitted and the solution shaken, the reduced catalase was oxidized and the original spectrum returned.

We have found that our dried catalase when in solution is reduced by adding $Na_2S_2O_4$ buffered at pH 7.5, as is shown by the two-banded spectrum

[2] J. B. Sumner and A. L. Dounce, *Jour. Biol. Chem.*, 127: 439, 1939.

[3] J. B. Sumner and A. L. Dounce, *Jour. Biol. Chem.*, 121: 417, 1937.

[4] D. Keilin and E. F. Hartree, *Proc. Roy. Soc. London*, B 124: 397, 1938.

[5] K. Zeile, G. Fawaz and V. Ellis, *Zeits. Physiol. Chem.*, 263: 181, 1940.

which appears, with a strong band at 560 mμ and a weaker and narrower band at 594 mμ. The dithionite can not be replaced by hydrogen sulfide or by a mixture of sulfite and bisulfite of neutral reaction. The two-banded spectrum given by reducing dried catalase with $Na_2S_2O_4$ is quite different from the two-banded hemochromogen-type spectrum which one obtains by adding $Na_2S_2O_4$ to a dried catalase solution which has been boiled. The latter shows bands at 560 mμ and 528 mμ, if light is made to pass through the precipitate which consists of completely heat-denatured catalase. This material of course is inactive towards hydrogen peroxide.

The two-banded spectrum of dried catalase treated with $Na_2S_2O_4$ is changed back to the original catalase spectrum on bubbling oxygen for a few minutes through the preparation, while the addition of a small amount of hydrogen peroxide causes the spectrum to revert instantly to the original catalase spectrum. The latter observation does not argue in favor of the mechanism of reaction of catalase with hydrogen peroxide proposed by Keilin and Hartree,[4,6] in which the iron was thought to be reduced by hydrogen peroxide and reoxidized by molecular oxygen. Dried catalase treated with $Na_2S_2O_4$ is qualitatively as active as dried catalase itself.

The behavior of dried catalase towards $Na_2S_2O_4$, which is probably its most interesting attribute, is summarized in Table 1, together with the corresponding behavior of undried catalase and boiled, dried catalase.

TABLE 1

Catalase Employed	Effect of Adding $Na_2S_2O_4$
Undried Crystalline Beef Liver Catalase	No Spectral Change
Dried Crystalline Beef Liver Catalase	A Two-Banded Spectrum Produced. Bands at 594 mμ and 560 mμ
Boiled, Dried Crystalline Beef Liver Catalase	A Two-Banded Hemochromogen-Type Spectrum Produced. Bands at 560 mμ and 528 mμ

Discussion

A possible explanation for the reducibility of dried catalase is that whatever change has been produced in the catalase molecule by drying includes a sufficient loosening of the linkage between iron and protein to permit $Na_2S_2O_4$ to reduce the iron. The question may be raised as to whether all of the lyophilized catalase has been changed in some way, or whether two thirds has been changed and one third left unaffected. To settle this point with certainty, much work of a physico-chemical nature might be necessary, but the behavior towards $Na_2S_2O_4$ makes it appear probable that none of the enzyme has escaped at least some change. Otherwise, one would expect to obtain mixed spectra of unchanged catalase and $Na_2S_2O_4$-treated catalase. At the present time it seems probable that the change produced in catalase by drying in the frozen state is of the nature of a very mild denaturation.

Another question of importance is whether the change in the enzyme molecule really occurs during the drying or during subsequent solution. The loss in activity and development of insolubility, which evidently is produced by atmospheric moisture acting over a period of time, indicates that at least part of the change may well occur during solution of the enzyme. It is even possible that eventually one might find a way to dissolve the enzyme so as to obtain it in the original condition.

Finally, one might wonder whether drying amorphous instead of crystalline catalase would give different results. This has not yet been investigated, but a solution of recrystallized urease was frozen and dried to an amorphous powder which was found to be completely water-soluble immediately after drying. It showed a high qualitative activity; quantitative determinations were not run. On standing exposed to the atmosphere the material gradually became insoluble and lost most of its activity during a period of about three months.

It should perhaps be noted that the work reported here deals with highly purified proteins of relatively high molecular weight,[7,8] so that any conclusions which one may wish to draw do not necessarily apply to protein mixtures or to certain low-molecular weight proteins like heart-muscle cytochrome C, which can be dried without apparent change.

Summary

Crystalline beef liver catalase has been dried by the lyophile process, and the properties of the dried preparation have been compared with those of undried crystalline beef liver catalase. The dried material is not crystallizable, possesses about one third the activity per dry weight of undried crystalline beef liver catalase, and in contrast to the undried material, its hematin iron can be reduced by $Na_2S_2O_4$ alone. It is thought that the process of drying, or the subsequent solution of the dried material, produces some change in the molecular structure such as a mild denaturation.

Alexander L. Dounce
Joe W. Howland

[6] D. Keilin and E. F. Hartree, *Nature*, 144: 787, 1939.

[7] J. B. Sumner and N. Gralén, *Jour. Biol. Chem.*, 125: 33, 1938.

[8] J. B. Sumner, N. Gralén and I.-B. Eriksson-Quensel, *Jour. Biol. Chem.*, 125: 37, 1938.

THE ENERGY COST OF STANDING IN HORSES[1]

THAT basal metabolism of the horse may be determined while the animal is standing is suggested by the fact that it appears to rest as comfortably standing as lying, and actually spends more sleeping time in the former than in the latter position. Resting metabolism data have been obtained on two pony mares trained from infancy to lie at command, and to tolerate a metabolism mask connected to a large spirometer of the Benedict-Collins type. While the difference is insignificant, metabolism of lying animals was found to exceed that of the horses in the standing position, as shown in Table I.

TABLE I

METABOLISM OF HORSES IN STANDING AND LYING POSITIONS

	Three year old filly Weight 700 lbs. (19 trials in each position)		Four year old lactating mare Weight 800 lbs. (9 trials in each position)	
	Standing	Lying	Standing	Lying
O_2 consumption, Cal/hr.	403.9 ± 19.8	432.3 ± 12.0	406.6 ± 40.2	448.3 ± 23.3
Ventilation rate, Liters/minute	86.8 ± 5.3	114.5 ± 2.9	69.9 ± 9.4	107.3 ± 3.7
Respiration rate, Resp./minute	13.3 ± 0.4	20.9 ± 0.5	11.6 ± 0.9	20.8 ± 1.1

The significant increases in ventilation and respiration rates observed in the lying position suggest that lying may interfere in some way with ease of respiration, and this in turn may increase metabolism. This, combined with the effect of the powerful suspensory and check ligaments which are doubtless an important factor in decreasing the energy cost of standing in horses, may explain the observation that metabolism of the horse is no higher, and indeed may be lower, in the standing than in the lying position. On the other hand, cattle and sheep, species which lack these powerful ligaments, exhibit an energy cost of standing of 10 per cent.[2]

The mares, on lying, usually assumed the characteristic position on one side of the chest in preference to that on the side with legs extended. Oxygen consumption ordinarily was determined once with the animal in the standing position, and once in a lying position on a given day. On some occasions the first determination was made with the animal in the standing position, while on other days this order was reversed. Lying down appeared to have no significant effect on pulse rate of the animals.

Since oxygen consumption of horses is no greater when the animals stand than when they lie, the latter position offers no advantage over the former in the measurement of basal metabolism.

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[1] Journal Series number 849.

[2] Warren C. Hall and Samuel Brody, *University of Missouri Agricultural Research Bulletin* 180, 1933.

SCIENTIFIC APPARATUS AND LABORATORY METHODS

SIMPLIFIED PHOTOMICROGRAPHY WITH A HAND CAMERA

THE relative ease with which good photomicrographs can be made, utilizing a box or folding hand camera and roll film, has not been generally recognized by visual microscopists, nor have the increased opportunities for making such photographs which are provided thereby because no darkrooms are required.

Most of the articles which have been published on simplified photomicrography recommend removal of the camera lens. The method described here consists in first independently focusing the microscope visually and then placing above (or behind) the ocular a roll-film or film-pack camera which has been focused at infinity or at another predetermined distance. Almost any type of personal camera can be used efficiently without mutilation and with the camera available immediately for other use. That the camera can be loaded in daylight and the films sent to the nearest drugstore for development and printing may be the chief advantages of the method to microscopists with no photographic facilities or experience.

The optical efficiency of this method is high if used properly; the definition of the pictures will be equal to that obtained with professional procedure and equipment. The optical considerations involved and the basis of the specific procedure that should be followed to obtain satisfactory results will be discussed more completely in a forthcoming article in a photographic journal. The Ramsden disk or eyepoint of the microscope should lie on the middle of the front surface of the camera lens ("vertex focus"), in which case the effective aperture of this lens is extremely small and the common optical aberrations become

negligible. A simple one-element lens such as those of inexpensive cameras is actually preferable in this case. As the camera lens is withdrawn from the eyepoint so that an appreciable lens aperture is used, chromatic aberrations are the first to have influence, but are practically eliminated if a color filter is used. The image degradation increases more rapidly as the camera lens is lowered below the eyepoint, however, than when it is raised.

Focusing the Camera

Since most people accommodate somewhat in focusing a microscope it is preferable to set the camera focus at twenty-five feet rather than at infinity and a fixed-focus camera is quite satisfactory. With a focusable camera it is preferable to determine the correct distance setting for the individual by making a series of negatives. Some individuals place the images as close as ten feet, but the distance may be variable, especially with young people.

By far the best procedure is the use of a telescope with a cross hair or other graticule in its eyepiece which has been carefully set for the individual focus. The focus of the entire telescope can then be set for the distance setting of the camera, such as infinity, by focusing out of the window. If the microscope image is focused through the telescope, even though this over-magnified image seems very poor, every worker may obtain identically excellent photomicrographs. If used by only one person a crude and simple telescope will be satisfactory if it will stay in adjustment, since it need be focused only once. A very satisfactory instrument was made by holding a 48-mm microscope objective by a cork in a metal tube so that the normal front of the objective faced the other end of the tube. A homemade graticule was held in the tube by another cork and this was viewed by a magnifier from a dissecting stand at the other end of the tube. If a micrometer microscope ocular is used, the lower lens should be screwed out.

Magnification

Since the camera lens determines the bellows length, with a hand camera the magnification obtained in the picture is always a fraction of the apparent (or catalogue) magnification; with a Kodak 620, for instance, the magnification is one third of the objective × ocular value. The effect is to reduce the magnification of the ocular. Therefore, only high-power oculars are satisfactory for photomicrography with a hand camera, oculars of 25× or more being most suitable.

Disadvantages

Since the definition of the photomicrographic image properly taken with a hand camera is equal to that taken by more expensive and elaborate outfits, and the procedure is simple, there must be disadvantages that have prevented its widespread adoption. There are only three important ones.

(1) The camera lens must be scrupulously clean, since any dust or marks on the lens will appear on the negatives, enlarged and slightly out of focus. (2) When the lens is at the eyepoint the whole visual field of the microscope is taken so that the picture is surrounded by a ring of out-of-focus detail, whereas in photomicrography on a large bench only the central portion of the field is taken. Raising the camera restricts the field, but a slight loss in general definition also occurs. (3) The ease with which a flare spot is produced in the picture is the greatest disadvantage of this method. In some cases it is unavoidable. The bright back lens of the objective may be imaged on the negative as a spot of increased density. With most of the cameras tested the flare image was in best focus at the same camera height that gave best image definition, *i.e.*, with the vertex focus at the eyepoint. As the camera is raised, the flare spot goes out of focus fairly rapidly, but some definition is lost.

Where critical definition is not required the camera can be raised considerably above the eyepoint to control the last two defects.

Apparatus

For this method the camera must be supported independently of the microscope and have no contact with it. When a record is unexpectedly needed the microscope should be tilted horizontally and the camera blocked up behind it at the required distance and along the same axis.

A stable, convenient apparatus can be set up from regular laboratory equipment with relatively little labor and at small cost. The ordinary $\frac{3}{8}$- or $\frac{1}{2}$-inch laboratory support rod is not sufficiently sturdy. A commercially obtained 19-mm vertical support rod and base plate can, however, be used. The right-angle clamp should fit this rod with a round hole rather than a V groove. The whole device is illustrated in Figs. 1 and 2. The base plate can, of course, be screwed directly to a bench or table top. Three small pieces of felt or adhesive tape are stuck underneath the corners of this board to act as feet.

The camera support (D or E of Fig. 2) consists of a wooden block stapled to the top of a 7-inch laboratory extension ring support. For folding cameras a rectangular hole, having the same dimensions as the folding flap of the camera front, is cut in the center of the board. The camera can easily and quickly be pushed into the same position each time, using one end of the hole and a strip of wood at one side as guides. The face of the board and the side of the strip should be covered with cloth to protect the

camera. Two right-angle hooks are placed opposite each other at each end of the camera. The camera is fastened down firmly by slipping two wide elastic bands over the hooks. It can thus be removed for other use or replaced in a few seconds.

To support non-folding cameras, holes are drilled into the board to provide for protuberances on the face of the camera (support *D* of Fig. 2 was made for a Brownie).

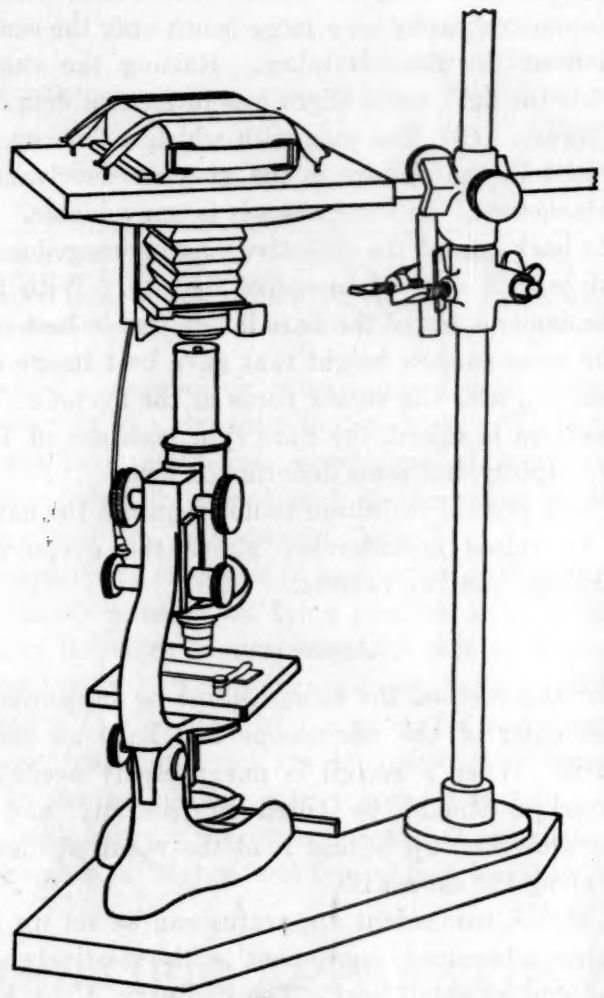

FIG. 1

By placing another clamp below the right-angle clamp that supports the camera ring, the camera can be swung out of the way when not in use. Moreover, by placing a short vertical rod (Fig. 2 C) as shown in Fig. 1, the camera is instantly realigned when its supporting clamp hits the vertical rod.

EXPOSURE CONTROL

The camera shutter is imaged on the field especially if the camera lens is slightly raised above the eyepoint, and this will vignette at very short exposure times to give apparent uneven field illumination. It is perfectly satisfactory when used with a watch on time exposures. A simple but satisfactory procedure for black-and-white pictures is to use a "blinker connection" such as is sold for Christmas-tree lights and to count the number of flashes to determine the exposure time.

DIRECTIONS

After the apparatus is set up and it is desired to photograph some specimen under the microscope, the procedure is as follows:

Determine the eyepoint of the microscope (illuminated disk of minimum diameter) with a piece of white paper.

Focus the camera at 25 feet or other predetermined distance.

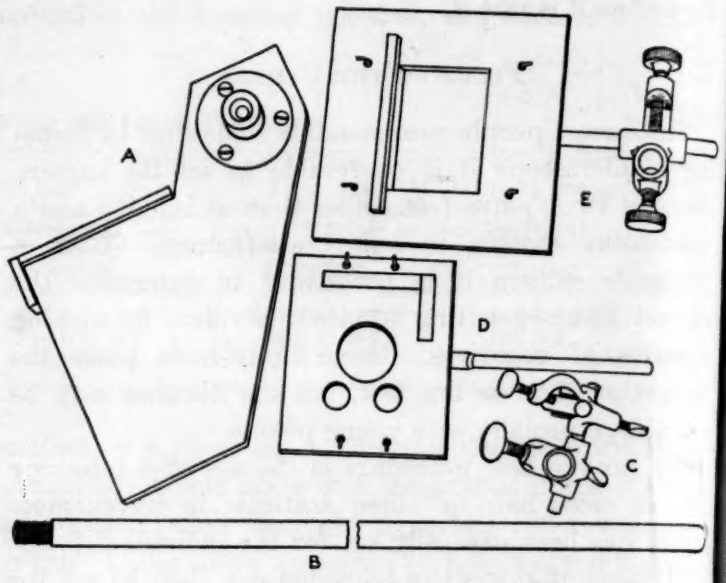

FIG. 2.

Lower the camera to make the front surface of the lens coincide with this point or be slightly above it.

Bring the lower clamp up to the other one and clamp it.

Loosening the upper clamp, align the camera lens with the ocular. This is best done the first time through the open back of the camera (containing no film) by looking through the lens. Tighten the upper clamp.

Rotate the lower clamp until the short vertical rod hits the upper clamp and again tighten it.

The camera can then be swung aside and a light-tight connector attached to the lens.

Focus the microscope visually, preferably through a telescope.

Swing back the camera and take the picture.

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